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ABSTRACT

The paper describes a variety of analytical difficulties facing prospective users of the first follow-up of the National Center for Education Statistics National Longitudinal Survey of the High School Class of 1972 (NLS) and suggests some possible approaches to coping with these. The primary focus is on the causes and consequences of selective item non-response in the first follow-up survey. Coding schemes used to flag this non-response and alternative approaches to estimating values for missing data are discussed. An examination of the special codes used for routing-pattern errors and missing data leads to the proposal of preparation of an analysis-oriented data file to parallel, but not replace, the existing documentary file. Certain coding modifications are mentioned which might be implemented for such a file. An examination of patterns of item non-response leads to the conclusion that the questionnaire's content and format, especially requests for detailed and/or private information, complex routing patterns, and a layout better suited to personal interviews than to mail-out collection, are probably responsible for some item non-response. Possible modifications that might reduce item non-response in future follow-up surveys are suggested. Review of several approaches to adjustment for missing data leads the authors to recommend a specific imputation procedure for data already collected. Also described are some possible methodological studies aimed at testing the effects of data assignments upon characteristics of the present NLS data base. (Author/RC)

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# ITEM-NONRESPONSE IN THE FIRST FOLLOW-UP SURVEY OF THE NATIONAL LONGITUDINAL SURVEY OF THE HIGH SCHOOL CLASS OF 1972

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## ABSTRACT

The paper describes a variety of analytical difficulties facing prospective users of the first follow-up of the NCES NLS survey and suggests some possible approaches to coping with these.

The primary focus of this paper is on the causes and consequences of selective item non-response in the first follow-up survey. Coding schemes used to flag this non-response and alternative approaches to estimating values for missing data are discussed.

An examination of special codes used for routing-pattern errors and missing data leads us to propose preparation of an analysis-oriented data file to parallel, but not replace, the existing documentary file. We mention certain coding modifications which might be implemented for such a file.

An examination of patterns of item non-response leads us to conclude that the questionnaire's content and format, especially requests for detailed and/or private information, complex routing patterns, and a layout better suited to personal interviews than to mail-out collection, are probably responsible for some item non-response. We suggest possible modifications that might reduce item non-response in future follow-up surveys.

Review of several approaches to adjustment for missing data leads us to recommend a specific imputation procedure for data already collected. We also describe some possible methodological studies aimed at testing the effects of data assignments upon characteristics of the present NLS data base.

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# INTRODUCTION

The National Longitudinal Study of the High School Class of 1972 is an ambitious, costly effort by the National Center for Educational Statistics to trace the careers of a cohort of young persons during the years following high school. The large sample selected (over 20 thousand participants) and the long questionnaires (some 50 pages in the original wave and the follow-ups) lead one to believe that a treasure-trove has been created for researchers interested in following up schooling decisions and career choices by young persons.

The Educational Policy Research Center for Higher Education and Society is especially interested in the insights which could be gained by analyzing this data. The topics covered are central to its mandate to develop policy-relevant information about the dynamics of choices to continue one's education beyond high school, the ways education is financed, reasons for not continuing education beyond high school, and the work experience of both those who stopped their education with high school and those who continued.

Even a cursory examination of the summary of the first follow-up impressed staff members of the Center with the difficulty of proposing meaningful analyses of this information. The complexity of the questionnaire, the difficulty of tracing response patterns, and the rather uneven luck with obtaining information for selected questions has caused us to analyze carefully some of the possible pitfalls which lie in

the paths of analysts of the subject survey.

The extended methodological note which follows should be useful to users of the survey, the staff of NCES who may wish to commission various analyses of the data, and to planners of future large-scale surveys. We hope that it will stimulate an exchange between data users which will enhance the usefulness of the data, and will help them in economizing effort to obtain maximum results.

Overview of the paper.

The first follow-up survey of the National Longitudinal Study of the High School Class of 1972 (NLS-HS) covers the early postsecondary experience of the sample members. The data from that survey are flawed by high rates of non-response to many items. The problem is quite critical when response is very low on a very important item. For example, only some 60 per cent of those listed as eligible to answer the question gave the amount of their first-year expenditures for school tuition and fees.*

Such gaps in information can seriously damage efforts to trace the long-term school and work experiences of the 1972 graduates. It will be quite difficult to determine reliably what relationships exist between (a) the base-year (pre-graduation) circumstances, (b) the early postsecondary school-and-work experiences, and (c) the later experiences

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* See Table 3, item F46BA.

of the class of 1972. The links between (a) and (c) will be especially hard to establish given poor information about the intervening period. Continued low item response rates in future follow-up surveys, coupled with normal sample attrition, will further aggravate analytical difficulties.

Our analysis of the patterns of item non-response in the first follow-up survey has several objectives:

1. to determine what information is most affected by item non-response,
2. to locate probable sources of item non-response,
3. to suggest possible ways of reducing item non-response in future follow-ups, and
4. to examine and assess various approaches to adjustment for missing data.

We examined item response rates as published in the user's manual for the follow-up survey. We found that response rates were very low for several matters of the greatest policy importance. Among the most often omitted items were those covering income, financing education, and other "money matters;" reasons for past choices; future expectations; and, in general, details about experience.

We attribute much of the item non-response to the format and content of the questionnaire, especially to its complex routing patterns and a format ill-suited to self-administration. We also found that the coding scheme used to prepare the documentary data file (the "data of

record") creates analytical difficulties, often spuriously inflates item non-response rates, and hampers identification of valid responses.

We suggest possible revisions in the questionnaire, for use in future follow-ups. Since we presume its content to be justified by specific information needs, the suggestions are limited to matters of format and response options. We emphasize that our suggestions must be proven successful in field pre-tests before adoption for use in future surveys.

We suggest the preparation of an analysis-oriented data file, paralleling the documentary file. For this effort, we recommend imposition of judgments about the validity of some questioned responses, assignment of values for missing data, and appropriate recoding. We emphasize that the documentary file, and its present coding scheme, should be retained as the primary record of the first follow-up data.

Our appraisal of various ways of assigning missing data leads to a recommendation that the method employed by the University of Michigan Institute for Social Research, for its "Panel Study of Income Dynamics," is best suited for use with the NLS-HS data. We also recommend, however, that empirical studies of the effects of data assignment upon the NLS-HS data base should be conducted under NCES auspices.

This paper is organized in two major sections. The first contains our description and analysis of patterns of item non-response. The second contains our discussion of approaches to adjustment for

missing data.

The first section opens with a detailed discussion of coding problems, focusing particularly on the routing-error codes which created problems in calculating item response rates. The remainder of the first section contains our discussion of the kinds of information most affected by non-response and our (conjectural) analysis of the sources of non-response.

The second section opens with a short commentary on pro's and con's of data assignment, especially with regard to longitudinal studies. Our review and critique of treatments for missing data in seven large data bases follows. The section closes with a brief discussion of the need for methodological studies to assess the consequences of data adjustment for the NLS-HS data base, and suggestion of some possible avenues of exploration for such studies.

The issues in brief.

As noted above, analysis of the survey is complicated by (1) high rates of non-response for certain items, (2) the use of data codes which make the computation of rates of non-response difficult, and (3) ambiguities in answers probably due to some design features of the instrument used to collect the data.

Item non-response, which is apparently quite large for some items, is difficult to deal with in most complex analyses. Selective non-response to particular items could be motivated by respondent

characteristics not measured by sample-selection variables, and hence may require more complex adjustment procedures than those used to re-weigh the sample for questionnaire non-response.

Adjustments of data for item non-response must rest on sophisticated guesses about what response would have been made had the respondent answered the question. Or, if an analyst decides not to modify the data base, the power of results is weakened, sometimes drastically, since the exact population to which generalizations can be made may be undefinable. Since conclusions may be affected by either course of action, decisions about data modification are important analytical issues.

Coding issues stem from the rules devised for transforming raw information--as supplied by a respondent--into analyzable "data." Rules for coding normally are devised with some particular objective in mind, and are fundamental to the processing and analysis of data. The intended uses of the data govern the coding policies; that is, the coded data incorporated in a file are products of a chain of policy decisions, and these generally result from certain intentions and assumptions on the part of those who devise the coding rules. Coding issues may arise, for any data base, when the objectives of coding are unclear or when different prospective users of the data base have different objectives for use of the data.

The fundamental policy of R.T.I., which devised and

implemented the coding rules for the NLS-HS data, was to avoid imposing judgments upon the data. They sought to retain as much of the diversity of "raw" responses as was consistent with the production of an interpretable data file. As a rule, this is a good policy, and we emphasize that R.T.I. is to be applauded for adopting it. However, in implementing this general policy, R.T.I. devised a coding scheme which, we think, makes it difficult for analysts to use the data.

Most important, the coding of non-response and errors in following routing patterns results in systematic misstatement of the proportions of usable response to items. This is a serious flaw because some prospective users may be dissuaded from attempting analysis, owing to artificially inflated non-response rates published in the user's manual, and because some people who were not eligible to answer certain items have been coded as "eligible but not responding."

Questionnaire design is the paramount issue for future waves of the NLS-HS survey. The First Follow-up Questionnaire has proved to fall short of its intended purpose in several ways; this may be due to:

(1) its physical layout, designed as if it were to be administered by a trained interviewer;

(2) complex routing instructions, an important source of confusion to respondents in the self-administered instrument;

(3) the response-options provided for many items which introduce unnecessary ambiguities for the respondent and probably underlie at least some of the skip-pattern errors;

(4) the lack of certain response options (chiefly "don't know"), which probably induces much of the item non-response and causes loss of (fairly) firm estimates of the extent to which respondents' ignorance of important matters may underlie their decisions, acts, and general experience;

(5) the number of pages (and hence the apparent length of the questionnaire), which is increased by wasteful use of space.* Since the propensity to respond is doubtless affected by the recipients' initial impression of how long the questionnaire is (which might be judged from the number of pages), compact spacing throughout is advisable;

(6) the booklet format of the questionnaire, which permits the respondent to enter at any point of his choice. Respondents who do not follow the prescribed item sequence, i.e., #1 to N, may well make skip-pattern errors and/or become so entangled in the various routing paths that they simply give up attempts to respond.

Procedure.

Since the technical discussion underlying the foregoing judgments about the methodological problems of the NLS-HS data base makes frequent and detailed reference to the "Base Year and First Follow-up Data File Users Manual" (R.T.I., 1975) for the survey, we urge the reader to obtain a copy of that document for use in following the discussion.

The technical discussion focuses on item non-response in

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* We presume the use of space was designed to ease processing of the questionnaire, an understandable objective, but not completely compatible with self-administered data collection.

the first follow-up survey. In the main, the base year survey is ignored because the mode of data collection used there will not be repeated and because item response rates there are generally high.

Our discussion covers only selected items from the First Follow-up Questionnaire. The items reviewed, we believe, are those relevant to policy issues likely to be discussed by federal policy analysts. These are enrollment, financing of postsecondary education, labor force participation and income, and reasons for decisions about postsecondary schooling. Some descriptive items, such as marital status and family background, are also considered. Other items may present equally difficult problems, but are not central to our interests.

This discussion is based on analyses of response patterns derived from the item response distributions published by RTI in the User's Manual. A supplementary paper, based on an examination of individual records from a special run of the public-use data tape, is now in progress.*

## CODING

### Relation to item non-response.

A discussion of coding problems must precede our comments on the core issue of item non-response because RTI's coding scheme

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* The supplemental investigation is a cooperative effort between our group and the College Entrance Examination Board.

makes it very difficult to calculate accurate estimates of non-response. RTI has created a complex set of codes to represent the variety of circumstances under which a clearly usable answer was not obtained. The codes on certain questions determine whether or not a person was counted among those eligible to answer other questions. Since the usable response rate for any item is based on the number of people eligible to answer the question, and since the way codes are used can inflate that base, calculated item non-response is very strongly influenced by the coding system. Mechanical application of decision rules, which exclude only cases carrying certain codes from among item eligibles, often leads to overstatement of the number of people eligible and, therefore, to understatement of the usable response rate. *

Description of special codes.

To follow the technical discussion, the reader must be acquainted with certain RTI codes and their use. We describe them briefly, but advise the reader to augment this by studying pages 22 through 30 of the User's Manual.

Routing-error "flag" codes. Respondents to the First Follow-up Questionnaire were not required to answer every question. Certain questions, called "routing items," direct the respondent to other

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* As RTI indicates with "The effect of this coding for non-response is to overestimate the illegitimate non-response... This implies that the user should be quite careful in interpreting the /non-response/ codes." (User's Manual: 29-30)

questions which he should answer. This is done by use of an instruction, "Skip to question --," which is keyed to one or more of the response options for the routing item. Ideally, the respondent should answer the routing question and all questions to which he is led by the instruction, but should not answer any questions he had been routed around.* In practice, respondents often failed to meet this ideal response pattern: forty-three per cent of the first follow-up respondents made at least one error in following routing instructions.**

Routing errors occur when a person answers the routing question and then fails to follow the "SKIP" instruction as directed. There are several ways such failures can occur, and RTI has devised a series of "flag" codes for routing-items to indicate that there is something wrong with either the routing-item response or subsequent responses.

Questionable routing-item answers are flagged by adding 20, 40, or 60 to the basic response code, depending on the type of inconsistency. Twenty (20) is added for respondents who answered some subsequent question they were directed to skip. Forty (40) is added when subsequent questions that were to be answered were left blank. Sixty (60) was added for respondents who made both errors, that is, answered a question they were directed to skip and failed to answer others they

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* Routing items may lead the respondent either to or around blocks of several questions.

** See User's Manual Table 6, "Quality Indices--Routing Questions" (p. 31).

were to have answered.

Non-response and unusable response codes. Several special codes were used to mark absence of a usable response. Three separate kinds of non-response are distinguished, and there are four codes for unusable responses.

The non-response codes are:

(a) Code 99 (LEGITSKIP), used for respondents who properly skipped an item they were routed around, as well as for 1,048 respondents to the base-year survey who returned no follow-up information.

(b) Code 93 (PARTIAL RESPONSE), used for non-response on a particular item, which is part of a set of related items, * when other items in the set were answered.

(c) Code 98 (BLANK), used to code item non-response when neither code 99 nor code 93 apply. (The "residual" nature of this code underlies many difficulties in calculating item response rates, as discussed below.)

The four unusable response codes are:

(a) Code 94 (DON'T KNOW)

(b) Code 95 (OUT OF RANGE)

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* For example, the item "Needed to earn money to support my family," which is one of 17 reasons listed separately in item F24 (Why did you NOT continue formal education after high school).

(c) Code 96 (MULTIPLE RESPONSE)

(d) Code 97 (REFUSED ANSWER)

The uses of these "garbage" codes are fairly straightforward, although we comment below on the possibly misleading labels.

"Garbage" codes rarely apply to significant proportions of response and create few analytical difficulties, in contrast to the "flag" and "non-response" codes.

## ANALYTICAL DIFFICULTIES ARISING FROM ROUTING-ERROR CODES

The routing-error codes, as used by RTI, create analytical difficulties described below.

Incomplete "flagging."

The simplest instance is the lack of routing-error "flags," comparable to those used for routing items, for conditional items.* In the published response distributions for conditional items, answers that are inconsistent with the routing-item responses are not distinguished from those that are consistent. This makes it impossible to count the number of "clean" (certainly usable) responses directly from the distributions. To determine whether or not conditional- and routing-item

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* Conditional items (questions) are those for which an answer is expected only on condition that a particular routing-item response was given. That is, conditional items are those to which a person is directed by the routing (or, SKIP) instructions keyed to routing-item responses.

responses are consistent, one must perform special computer runs in which both responses are compared. This adds a data-processing step which is costly, and provides extra opportunities for analysis error. The lack of "flags" for conditionals is particularly troublesome when a routing item controls entrance to blocks of conditional items. In such cases, it may be necessary to check consistency for every item in the block in order to find the origin(s) of a routing-item "flag" code.

The absence of "flag" codes on conditional responses is a serious flaw, not only because it requires extra data processing but also because potential users cannot easily assess the adequacy of the data base by reference to the User's Manual. Some studies may not be undertaken because prospective analysts are dissuaded from attempting them.

Response eligibility.

A far more serious analytical difficulty created by the routing-error codes is inflation of the number of people counted eligible to answer conditional items. As previously noted, this affects calculated item response rates, sometimes quite markedly.

RTI's published distributions count as "eligible to answer" all those not coded 99 (LEGITSKIP); that is, unless definitely ruled out of the eligible pool, a person is deemed eligible. The number of LEGIT-SKIPs for conditional items is determined by subtracting only the "clean" SKIP responses* from the total sample size. Any routing-item responses

* Plus the constant 1,048 people who returned no questionnaire.

which are error-coded are, therefore, deemed eligible for subsequent conditional items.

To illustrate:

Item F23 asks "Since leaving high school, have you attended any school...?" Response options are YES and NO. If YES is marked, the respondent is directed to "SKIP to q. 25." If NO is marked, he is expected to answer items F24A through F24Q (which are a list of reasons for not continuing formal education) and to exit Section B.*

By RTI's procedure, only those who gave a "clean" NO to F23 are ruled ineligible (LEGITSKIP) to answer item F25, which asks "Were you taking classes or courses at any school during the first week of October, 1973?" There were 5,447 "clean" NOs to item F23, and the published LEGITSKIP for item F25 is 6,495, or 5,447 + 1,048. There were, however, a total of 2,360 additional NOs bearing routing-error "flag" codes, and none of these are ruled ineligible to answer item F25.

Since they are not ruled out of the eligible pool for F25, they are treated as eligible. There were 776 cases coded 42 on F23 (NO to F23 and failed to answer any items in F24A-F24Q** and properly exited from Section B, as directed in F24). These seem clearly ineligible to answer further questions about postsecondary schooling, yet were counted eligible for F25--as well as all further items in Section B--

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* Section B is that portion of the questionnaire which deals with post-secondary education and training.

** Items F24A through F24Q are a list of reasons for not continuing formal education after high school.

simply because of the routing-error code for item F23.

The use of code 42 for item F23 indicates that these 776 people answered no subsequent questions in Section B. * They must, therefore, have been included among those coded 98 (BLANK), for all remaining items in Section B. Since they contribute to the eligible pool, but not to the usable response pool, for all these items, their dubious inclusion will decrease the apparent usable response rates.

The rates can be calculated, of course, after subtraction of 776 from the eligible pool for any item from F25 through F47GB (the end of Section B), but it seems undesirable that such an adjustment must be made to compensate for the vagaries of response coding.

This case illustrates one of the simpler difficulties which arise from the routing error codes. Where a sequence of routing-items precedes a conditional item, adjustment of the eligible pool to compensate for such dubious inclusions requires extensive computer analysis of response patterns. The supplementary paper mentioned above will present an attempt to perform such an adjustment. ** It will illustrate the difficulties faced by analysts as a result of routing-error coding.

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* See the listing for Q23, code 42, and the footnote, on page 1 of Appendix E.1, User's Manual.

** For a small block of 7 items covering schooling costs for the first year after high school, which are near the end of questionnaire Section B.

Differing effects on calculated response rates.

All of the routing-error codes affect calculations of item non-response from the published distributions. Since the "flag" codes have different meanings, their impact on estimated item non-response will vary. In some cases, inclusion of "erroneous" responses among the eligibles can increase the estimated item response rate, in others (as in the illustration above) it will decrease the rate.

Working only with the distributions published in the User's Manual, we have tried to assess the influence of various error-codes on response rates. For selected routing items, we related the number of each kind of routing error to the proportion of BLANKS for subsequent conditional items. Our objective was to determine how many of the BLANK responses might have been contributed by people who erred in following the routing pattern. This effort yields some suggestions for modifying the data base to reduce the analytical difficulties posed by the routing-error codes.

Table 1 shows the contribution of error-coded responses (to each routing item) to total response for the routing item and its first conditional item. * The content of the entries varies by code because of the different possibilities for contribution entailed by each code. **

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* That is, the next item in the numerical sequence.

** Refer back to page 11 for the explanation of these codes.

TABLE 1

ROUTING-ERROR CASES AS A PROPORTION OF ROUTING- AND CONDITIONAL-ITEM ELIGIBLES

| Routing Item No. | Content | Code 20 | | |
|---|---|---|---|---|
| | | Number of Cases | Per Cent of Item Eligibles | |
| | | | Routing Item | Conditional[a] Item |
| F 2: | Complete high school? | 14 | 0.07 | 0.07 |
| F 7A: | Marital Status 10/73 | 251 | 1.2 | 4.1 |
| F 8A: | Number of children, if any | 52 | 0.9 | 1.4 |
| F 13B: | Anyone discuss borrowing? | 21 | 0.1 | 0.3 |
| F 21: | Participated in a training program since high school? | 268 | 1.3 | 5.5 |
| F 23: | Any kind of schooling since high school? | 1,299 | 6.1 | 16.0 |
| F 25: | Attending any classes 10/73? | 226 | 1.4 | 1.9 |
| F 28B: | Field of study 10/73 academic or vocational? | 163 | 1.3 | 1.4 |
| F 29A: | Attending any classes 10/72? | 449 | 2.8 | 8.9 |
| F 30: | School 10/72 same as school 10/73? | 435 | 3.1 | 8.9 |
| F 48A: | Working 10/73? | 285 | 1.3 | 3.5 |
| F 54A: | Working 10/72? | 361 | 1.7 | 3.6 |

[a] "Conditional item" is the first item conditioned on the routing item; i.e., the next item in the numerical sequence as given in the User's Manual distributions.

TABLE 1 (Cont'd)

ROUTING-ERROR CASES AS A PROPORTION OF ROUTING- AND CONDITIONAL-ITEM ELIGIBLES

| Routing item No. | Content | Code 40 | | | |
|---|---|---|---|---|---|
| | | Number of Cases | Per Cent of Item Eligibles | | Total Non-Response Rate--Conditional Item(s)[b] |
| | | | Routing Item | Conditional[a] Item | |
| F 2: | Complete high school? | 1,727 | 8.1 | 8.1 | 19.1 |
| F 7A: | Marital status 10/73 | 101 | 0.5 | 1.7 | 44.6 |
| F 8A: | Number of children, if any | 37 | 0.6 | 1.0 | 71.9 |
| F 13B: | Anyone discuss borrowing? | 183 | 0.9 | 2.4 | 37.3 |
| F 21: | Participated in a training program since high school? | 54 | 0.3 | 1.1 | 26.7 |
| F 23: | Any kind of schooling since high school? | 780 | 3.7 | 9.6 | 20.4 |
| F 25: | Attending any classes 10/73? | 47 | 0.3 | 0.4 | 26.2 |
| F 28B: | Field of study 10/73 academic or vocational? | 394 | 3.2 | 3.3 | 27.5 |
| F 29A: | Attending any classes 10/72? | 305 | 1.9 | 6.0 | 46.4 |
| F 30: | School 10/72 same as school 10/73? | 357 | 2.5 | 7.3 | 72.4 |
| F 48A: | Working 10/73? | 602 | 2.8 | 7.5 | (17.1[c]<br>(11.3[d] |
| F 54A: | Working 10/72? | 1,164 | 5.4 | 11.7 | (18.2[c]<br>(13.3[d] |

[a] "Conditional item" is the first item conditioned on the routing item; i.e., the next item in the numerical sequence as given in the User's Manual distributions.

[b] Where more than one conditional item is contained in the skip pattern, based on averages for all. Where "Partial Response" is among categories, figure shown is the sum of BLANK and PARTIAL RESPONSE.

[c] Average for "Reasons for not working" set, excluding "Going to school."

[d] Rate for "Looking for work...," item 48C or 54C.

TABLE 1 (Cont'd)

ROUTING-ERROR CASES AS A PROPORTION OF ROUTING- AND CONDITIONAL-ITEM ELIGIBLES

| Routing Item No. | Content | Code 60 | |
|---|---|---|---|
| | | Number of Cases | Per Cent of Item Eligibles Routing[c] Item |
| F 2: | Complete high school? | 0 | 0 |
| F 7A: | Marital status 10/73 | 0 | 0 |
| F 8A: | Number of children, if any | 0 | 0 |
| F 13B: | Anyone discuss borrowing? | 0 | 0 |
| F 21: | Participated in a training program since high school? | 0 | 0 |
| F 23: | Any kind of schooling since high school? | 521 | 2.4 |
| F 25: | Attending any classes 10/73? | 0 | 0 |
| F 28B: | Field of study 10/73 academic or vocational? | 0 | 0 |
| F 29A: | Attending any classes 10/72? | 230 | 1.4 |
| F 30: | School 10/72 same as school 10/73? | 0 | 0 |
| F 48A: | Working 10/73? | 83 | 0.4 |
| F 54A: | Working 10/72? | 123 | 0.6 |

[c] See text discussion regarding omission of "proportion of conditional-item eligibles."

Code 20 implies that a response was given. In the table, its contribution to the pool of conditional-item eligibles is also, therefore, its contribution to the published response rate. *

Code 40 implies that no response was given. ** Its contribution to the conditional-item eligible pool is also, therefore, its contribution to non-response. We show the non-response rate for conditional items for comparison with the contribution of "erroneous" responses to the conditional-item eligible pool.

Code 60 designates a combination of routing errors. For reasons to be discussed, we omit its contribution to conditional items.

We can compare the contributions, to routing and conditional items, made by those who erred in following routing instructions. This gives us some notion of the impact of each error code on conditional response rate.

Code 20's contribution to analysis difficulties. Consider code 20 for item F23 in the table. The 1,299 people whose response to F23 was questioned because of later inconsistent responses were only six per cent of all those eligible to answer F23. These same people can

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* For the present purpose, we have assumed that the questioned response was given for the first conditional item. This is not necessarily true, since code 20 implies at least one erroneous response somewhere among several conditional items.

** Where there are blocks of conditional items, code 40 means none were answered.

account for sixteen per cent of those answering the conditional item (F24: Reasons for not continuing education after high school). Their disproportionately large contribution to the eligible pool (and the response rate) for F24 shows that a small minority of erring respondents to F23 contributed heavily to the responses for F24. If these cases were ruled out of the data base for F24,* the size of the eligible pool would drop from 8,118 to 6,819 and the usable response rate for item F24A would drop from 79.8 per cent (6,481) to 76.0 per cent (5,182). If they were retained in the eligible pool for F24A, but dropped from the usable responses, the usable response rate would drop from 79.8 per cent to 63.8 per cent. Obviously, where routing-item code 20s make a disproportionately large contribution to the conditional-item eligible pool, they have a significant impact on the calculated response rates.

The analyst's interpretation of the questionable responses can exert an important influence on his results. One cannot be sure which of the two inconsistent responses (routing- or conditional-item) is true. Therefore, some decision must be made by the analyst, but whatever decision he makes will affect response and non-response rates. As we have just shown, complete elimination of code 20 cases from the conditional-item eligible pool will result in a decreased response rate. This happens because the number of BLANK cases remains constant but constitutes a

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* This is not a recommendation. However, we suppose some analysts might wish to work only with unquestionably valid responses.

larger proportion of the reduced eligible pool. Conversely, retention of code 20s will artificially increase the usable response rate by the extent to which genuinely erroneous conditional item responses are represented among those coded 20 on the routing item. *

To further compound the ambiguity surrounding decisions about inclusion or exclusion of code 20s, it can happen that a code 20 on the routing item may result from a "garbage coded" response to a conditional item. In such a case, the number of usable responses will not be increased, but the eligible pool will, and the usable response rate will be somewhat reduced. In the first follow-up survey, this seems to be an exceptional case, but we comment on it later in remarks about code 94 (DON'T KNOW).

To this point in the discussion of code 20 responses, we have considered only the case where an inconsistent response is made, i.e., some actual answer is given. But those coded 20 on a routing item need not have answered all conditional items in a block of related items. A flag code 20 was assigned if there was at least one inconsistent response following the routing item.

Thus, in our example above, some of those coded 20 on item F23 might have answered (say) item F24B, but not F24A. In that case,

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* We assume that some of those coded 20 on the routing item erred in marking the routing item, and that others erred in marking the conditional item; which response is really erroneous is not certain.

they are considered eligible for F24A but coded as a PARTIAL RESPONSE (code 93). *

In discussing the example, we said that inclusion of code 20s would increase the "usable" response rate, and decrease the non-response rate for a conditional item. Now we must modify that statement. In the situation we are now considering, the code 20s for F23 become code 93s (PARTIAL RESPONSE) for item F24A, and, of course, the result is to increase the non-response rate while decreasing the usable response rate.

The analytical difficulties presented by the code 20s are mind-boggling. Consider, for example, what we conceive as a "worst case" situation: An analyst is interested in certain attributes of people who claimed they stopped their education with high school because their plans did not require more education. Item F24L is his key selection item, because it gives that reason.

On the basis of the User's Manual distribution, he finds that the usable response rate is 79.6 per cent, and that total non-response (BLANK plus PARTIAL RESPONSE) is 20.2 per cent. ** He further sees that the people of interest to him (those answering "applies to me" concerning the stated reason) number 2,729 cases, and that there are only 17 "garbage code" cases. He rules out the 3,730 who answered "does

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* Despite its label, code 93 means a form of non-response, as described above.

** See item F24L in our Table 3 (appended). The other 0.2 per cent is accounted for by other codes and rounding error.

not apply to me" (since they lack the controlling characteristic), and must decide what to do about the 1,642 non-respondents.

We know that there are 1,299 "code 20" respondents spread somewhere throughout this distribution, none of whom is coded either BLANK (98) or LEGITSKIP (99).* But because there are no "flag" codes for routing-item errors on item F24L, neither we nor the prospective analyst know how these doubtful responses are scattered among the coding categories. They might all be among the people of interest ("applies to me"), or all might be loaded on other response codes, or (more likely) they may be variously distributed over all codes other than 98 and 99.

Depending on the actual distribution of the code 20s among the responses to F24L (which, recall, can include the PARTIAL RESPONSE code 93) and our analyst's decision about whether the code 20s are or are not valid responses, the actual number of cases available for his study could be as many as 2,729 (all "applies to me" responses) or as few as 1,430.**

We need not carry this "worst case" illustration further, since the analytical difficulties faced by our fictional analyst must be

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* Code 20 in F23 guarantees this, by its definition. See User's Manual Appendix E.1, Q23, codes 21 and 22.

** Most of those coded 20 (1,079) gave a NO response to item F23 (Any school after high school?), and should have answered F24L on that account. However, the truth of their response to F23 is in doubt because of later responses which suggest they had some postsecondary education; hence their eligibility for F24L, and the hypothetical study, is in doubt.

evident. Tracing the F24L responses of those 1,299 doubtful cases will require several crucial analysis decisions, hours of programming preparation, substantial computer costs, and possession of the data tape. All of this must be done before the researcher can even decide whether to go ahead with his projected study.

These illustrations of the possible difficulties stemming from code 20 on item F23 by no means exhaust the matter. The reader can examine other routing items in Table 1 to see the number of equally difficult instances implied by the proportions of conditional-item eligibles. We think that where the figures in the paired cells differ markedly, the analyst will face trouble. Seven of the twelve routing items listed in Table 1 fit this criterion, and these seven items directly affect a total of 234 other items in the First Follow-up Questionnaire. *

What is to be done about code 20s? Having elaborated the analytical difficulties posed by RTI's use of code 20, we feel obligated to suggest some remedy for the situation. Our first thought was that code 20 cases on critical routing items (like F23: Any school after high school) might be deleted from the follow-up data base. This seems impractical, however, because to delete only F23 code 20s would shrink the data base by 6 per cent, and the cumulative effect of dropping others

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* By count of the items within the routing patterns of items F7A, F21, F23, F48A, and F54A. Items F29A and F30 are coupled, as screens, with F23. See User's Manual Appendix E.2.

would reduce it still farther.

Instead, we recommend imposing more judgments about the validity of 20-coded responses. We think it possible to estimate the probable truth or falsity of such responses by examining subsequent response combinations.* Such a judgmental reassessment of routing-item "error" responses can lead to reclassification of responses, largely eliminating code 20s as a response category but not removing them from the data base.

In the later discussion of questionnaire format as a source of data problems, we suggest some ways to forestall the occurrence of code 20 cases in future waves of the survey. Some of these involve changes in the physical layout of the questionnaire, others involve changes in the response options to various items.**

Code 40's contribution to analysis difficulties. We have already given one example of the impact of routing-error code 40.*** We showed that 776 people so coded for item F23 (Any schooling after high school) were inappropriately carried through to the eligible pools for

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* We have used such a procedure in the recomputation of eligibles for items 46A and B (first year school costs), the results of which will be described in the supplemental paper now in progress.

** We are aware that such changes may make data non-comparable across survey waves. We discuss this matter in the section on formatting.

*** Failure to answer conditional items.

every item from F25 to F47GB.

The use of code 40 will not always cause such harm. When routing instructions are used to shunt some people around one or a few items in a sequence which is otherwise applicable to all, the eligible pools will not be unduly inflated. For example, item F7A (What was your marital status, as of the first week of October 1973?) routes the never-married around questions about the date of marriage and whether or not the respondent had any children.* All respondents are then expected to answer the next question (F9: In October 1973, were you financially dependent...). A code 40 on item F7A indicates that no information was given in the conditional items, but those so coded are obviously eligible to answer them and to answer later questions. Calculations of item response rates are not affected in such a case.

As we see it, no useful information is added by code 40, since it does not appear to flag a genuine routing pattern error. Without this flag, those cases would still be in the eligible pool for conditional items to which the "erroneous" response directs them, and would be counted BLANK for those items.

But as we have shown, the absence of this flag would prevent false inclusion of the 40-code cases in the eligible pools for items they

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* Possibly unwisely in the latter case, since parenthood does not require marriage and the estimated illegitimacy rate now runs to about 13 per cent of all U.S. births. Responsibility for children, legitimate or not, doubtless affects decisions about work and school.

are not eligible to answer. So, code 40 appears to contribute only mischief and data-processing confusion.

The data in Table 1 reinforce our belief that code 40 should be eliminated. The percentages there show that code 40s consistently contribute a large share of the conditional-item eligible pools. Although in several cases they account for almost half of the total non-response for conditional items, they are always well within the general pattern of non-response. In other words, when the use of code 40 is not doing harm, it adds nothing to our understanding of conditional responses.

We recommend discontinuation of the 40-code flag.

Code 60's contribution to analysis difficulties. As shown in Table 1, respondents with mixed routing errors (code 60s) make up a rather small part of the data base. Even if there is no overlap among those making such errors (an unlikely event), they would make up no more than 4.5 per cent of all respondents.

If there is no overlap among respondents making code 60 errors on the crucial status items* listed in Table 1, deletion of all such cases would reduce the available data base to 20,393, for an overall follow-up response rate of 91 per cent. If there is complete overlap (i.e., all subsequent code 60 errors were made by those so coded for

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* F23: Any schooling after high school; F29A: Enrolled in October 1972; F48A: Did you hold a job, first week October 1973; F54A: Did you hold a job during October 1972?

F23), the data base would be reduced to 20,829, for an overall response rate of 93 per cent. Since there is probably partial overlap, the true effect would be to reduce the overall response rate to something between 91 per cent and 93 per cent.

Since there must be almost total uncertainty about the true enrollment or work status of code 60 respondents, their information must be considered highly unreliable and probably should not be used. Considering the reduction of analytical difficulties to be gained by deleting these cases, and the fact that an overall response rate of 91 per cent or better would be quite respectable, we think it advisable to rid the data base of these highly ambiguous cases.

Closing comments on routing-error codes. Our suggestions for treatment of the routing-error responses require a degree of willingness to intervene in the data which RTI rightly abjured. It is emphasized that we recommend such intervention only if NCES wishes to make available a parallel analysis tape, on an optional basis, to prospective users. In no event should the RTI documentary version be replaced by a modified data tape, since some users may prefer other treatments. The treatments thus far recommended would serve only to reduce the amount of ambiguous data and eliminate sources of analytical difficulty. They will not supply missing data for non-respondents, but they will help to fix more accurately the number of persons eligible to answer given items, and they should eliminate most of those cases for which data may be

supposed unreliable owing to respondent inability or unwillingness to follow instructions.

ANALYTICAL DIFFICULTIES ARISING FROM OTHER CODING

The influence of the routing-error codes upon the calculation of LEGITSKIP and BLANK is the major coding source of analytical difficulties, but there are others. This section is a rundown of miscellaneous observations about how the code structure might be altered to make analysis easier.

Code 98-BLANK.

We think that frequency counts listed as BLANK are inflated by factors other than the mechanical inclusion of routing-error codes. These are chiefly by-products of the instructions and response options given in the questionnaire.

An example of one extreme case will give the reader an idea of how BLANK counts can be inflated by such factors.

Items F11B, D, F, and H ask for information about the 1973 income of the respondent's spouse. The exact wording of item F11 is: "What is the best estimate of your income before taxes for all of 1973? If you are married, please estimate your husband's or wife's income in the second column provided. Do not include loans or gifts." Below the question is a list beginning with total income, then seeking source details: "from wages, salaries, . . . ," "scholarships, fellowships," and "other (for example, interest)." Two response columns are supplied, one

headed "Your Own Income," the other "Your Spouse's Income." Item F11 is not contained within any routing pattern, i.e., it is to be answered by all respondents.

The published response distribution for F11B (spouse's total income) lists 17,597 BLANKs and only 3,519 usable responses, for a usable response rate of 16.5 per cent. The large number of BLANKs is mainly a result of (1) the failure to condition response about spouse's income directly upon item F7A (What was your marital status, as of the first week of October 1973?) and (2) the format of the instruction and response for F11B, D, F, and H.

From the standpoint of machine processing, all respondents were eligible to answer items F11B, D, F, and H. Thus, lack of response in the "spouse" column was automatically entered as BLANK rather than LEGITSKIP.

In the absence of some instruction requiring a positive entry (such as, "Write NONE in the second column if you were not married in 1973"), blanks there are hard to interpret. They could mean that there was no spouse, or that there was a spouse who had no income, or that there was a spouse with income but the respondent can't estimate it, or that there was a spouse with income which the respondent won't divulge. The code category BLANK presumably includes all of these.

Second, positive entries are also hard to interpret. The instruction says "if you are married," implying "married at the time

you are filling out this questionnaire." Some respondents may have been married or divorced during the several months between the first week of October 1973 (the reference week in item F7A) and the date they completed the questionnaire, which ranges through February 1974. Entries of "spouse's 1973 income" from those married in the interim may have little or no bearing on their own education or work experience through late 1973. Those divorced in the interim (hence "not married now") presumably will have left the "spouse's income" column blank, even though their own education and work experience through late 1973 would have been affected by their (former) marital status.

The number of BLANKs for this item is thus affected by a number of formatting and data-processing factors. It is patently absurd to suppose that all respondents were indeed eligible to answer about a spouse. Therefore, we have used item F7A, items F7B and C (date married), and Census information to make a crude estimate that perhaps 4,050 respondents were married in 1973 and thus eligible to answer the question. The remainder (17,300) we treat as LEGITSKIPs.* On this basis, the number of BLANKs for F11B (spouse's total income) becomes only 531, and the revised usable response rate rises from 16.5 per cent to 86.9 per cent.

We think that the usable response rates for some other items

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* 17,300 excludes the constant 1,048 general non-respondents.

are affected in similar ways by the combination of instructions, response options, and automatic processing, although we did not attempt to revise the distributions for them. Our belief that usable response rates are sometimes understated lends a cheering note to this discussion, but the amount of work required to revise item F11B indicates that considerable effort will be required to reassess item eligibilities and recode cases from BLANK to LEGITSKIP.

BLANK and DON"T KNOW. The frequency of "DON'T KNOW" responses (code 94) is astonishingly low for all of the items we examined, while that for BLANK (and PARTIAL RESPONSE) is persistently high. We think that many of the BLANKs are probably a way of expressing "don't know" or related responses (e.g., "can't recall"). We are not, of course, able to reassign BLANKs to "DON"T KNOW" as we were for some reassignments to the LEGITSKIP category.

Inspection of the First Follow-up Questionnaire (but not an actual item count) indicates that appropriate "uncertainty" response options are provided for no more than half-a-dozen questions, even though the questions often ask for details, time-remote events, or facts about others which are not likely to be well-known to the respondents.

For future waves of the survey, we strongly recommend greater use of "uncertainty" options. This suggestion may be opposed on two grounds, (1) that it provides a loophole for denial of information and (2) that it merely substitutes one uninterpretable category for another.

We reject these objections because we think that "uncertainty" options will improve the amount of valid information (as distinct from technically valid data) obtained.

Experience shows that respondents will often supply a "firm" or positive answer even when, in fact, they do not have well-grounded beliefs, facts, or attitudes. They do this simply to "help" a researcher or to give the appearance of being well-informed. When they are not shown, by an offered response option, that "don't know" or the like is a perfectly acceptable response, they may create a "fact" on the spot in order to satisfy seeming demands. Inclusion of a "don't know" option would reduce the number of such responses. Since BLANK (non-response) is already available as a way to deny information, the addition of appropriate "uncertain" response options can only improve the validity of information.

On the second point, "don't know" or like responses are interpretable, but BLANK is not. Analysis of such responses can reveal the extent to which ignorance, apathy, and fading memory influence decisions and acts. "Don't know" and other "uncertainty" response options thus can provide a great deal of useful information which may otherwise be buried in the BLANK category.

Closing comment on BLANK. In a later section of this paper, we discuss methods available for estimating values to be assigned to BLANK responses. While some of those methods are rather sophisticated,

none are as desirable as preventing the occurrence of BLANKs. As we have shown, the high frequency of BLANKs has been induced partly by artificial inflation, partly by inappropriate questionnaire design, and partly by absence of response options that might be used. At least as much effort should be given to preventing future BLANKs as will doubtless be given to supplying missing data. Because overall response rates are likely to fall continuously over the duration of this (like any) longitudinal survey, we think that item non-response in the returns simply cannot be afforded from an analysis standpoint. Therefore, it is an urgent necessity that our (and others') suggestions be field tested and, if successful, implemented in future waves of the survey.

Response Option: "Does not apply."

The obverse of BLANK inflation is inflation of "usable" responses. We think the response option "does not apply to me" inflates rates of usable response for many items, among them some that may be the origin of 20- and 60-routing-error codes on critical routing items.*

The options "applies to me" and "does not apply to me" appear to have been used in lieu of more straightforward "YES" and "NO,"

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* Such as, F24 (Reasons for not continuing education after high school), F29B (Reasons for not continuing education right after high school), F48B (Why were you not working during the first week of October 1973), and F54B (Why were you not working during October 1973). Some other items which use "does not apply to me" are F31 (Reasons for changing schools), F35 (Reasons for changing academic field), and F38 (Reasons for dropping out of school).

probably to suit the wording of the questions involved. Our examination of response distributions leads us to think that fairly large numbers of respondents used "does not apply to me" as if it pertained to the whole issue raised by the question. For example, some who had not stopped their education after high school probably circled "does not apply to me" for at least a few of the reasons for stopping listed in F24. Their meaning of the response is that the whole matter of reasons for stopping does not apply to them; this is not, of course, the meaning intended by the questionnaire designer(s).

The use of "does not apply to me" as a response option probably has inflated the usable response rate for affected items. Perhaps worse, it would reduce the proportion of "applies to me" responses for any particular item so coded, thereby biasing conclusions about the importance of the reason (or whatever). As we have mentioned, its most serious and pervasive effect may be the creation of many 20- and 60-error-coded responses to routing items.

We think respondents should not be offered such an opportunity for error, and that analysts would be better served by the much less ambiguous YES and NO options. Questions can be worded to suit the options, rather than the converse, and we think they should be.

Code 93--PARTIAL RESPONSE.

This code is used when there is no response to an item which is part of a set of related items (e.g., reasons for dropping out of school)

and at least one item in the set has been answered. From an analysis standpoint, it is chiefly a nuisance. Data processing runs must be programmed to add the frequencies of PARTIAL RESPONSE and BLANK to tabulate the total non-response for items so coded.

The code is intended to describe the context in which the non-response occurred, differentiating those who failed to answer any items in the set from those who answered at least one. The code may be helpful in analyzing such matters as routing errors, but to the substantive analyst it is merely an obstacle in data processing.

The information contained in the code could be better conveyed to the substantive analyst by addition of a general item code on the data tape. Such a variable would use three codes, designating (a) response present for all subitems, (b) response absent for all items, and (c) response present for only some items.

Analysts interested in the context of non-response on a particular subitem could then sort on the general item code as a first processing step. Others not interested in the context could avoid programming for the addition of PARTIAL RESPONSE and BLANK. We think the latter are likely to outnumber the former among prospective users. We suggest that for their convenience--and to remove opportunity for error in calculating response rates--the special code 93 should be dropped, and those cases shifted to BLANK (98). Such a move would require creating the general item variable, as suggested.

Uncodable Responses--OUT OF RANGE (95) and MULTIPLE RESPONSE (96).

We think these two codes could be eliminated and the cases merged in a single code category designated UNCODABLE. Neither existing code tells us anything more than that a response was given which did not fit the coding scheme (unless we are interested in why it doesn't fit). We think few analysts will care why a response is uncodable, and for those who do* the unaltered RTI documentary tape would be available.

Our purpose in recommending the merger and re-labeling of these two categories is simplification of data processing. There are few cases in either category (with the exception of an apparent data-processing mistake for item F41CA: Number of credit hours attained after high school, for which 19,947 OUT OF RANGE responses are listed), and ease and economy of data processing seems more important than retention of details on a handful of cases.**

Comments on Category Labels.

In keeping with our insistence that opportunities for error be kept to an absolute minimum in the whole survey process, we think some response category labels should be altered because they misrepresent

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* Such as people concerned with possible modification of the NLS-HS questionnaire.

** RTI's tabulation error points out that it is advisable to hold the number of codes, hence the opportunities for error, to a minimum.

what is actually encoded.

BLANK does not include all BLANKs; there are also PARTIAL RESPONSEs and LEGITSKIPs. If PARTIAL RESPONSE were eliminated, as suggested above, the important distinction between BLANK and LEGITSKIP would be preserved and better characterized by use of MISSING or MISSING DATA instead of BLANK.

OUT OF RANGE does not include all out-of-range responses. Cases in that category are far better described by our recommended UNCODABLE, because OUT OF RANGE refers to cases which could not be processed by the machine-reading used to convert raw responses to coded data.

Truly "out of range" responses are listed by RTI in Table 5 of the User's Manual (which is itself mislabeled with the code 95 name although it has no bearing on that code category). Responses which fit the coding scheme but were deemed highly improbable are enumerated in Table 5. In the published distributions, these cases are separated from the code 95 OUT OF RANGE responses. The latter are likewise not included in the tallies given in Table 5.

Although the data tapes contain these "outliers" just as reported, the response distributions published in the User's Manual do not clearly distinguish them as a separate category of dubious responses, and present an unneeded opportunity for error. We think that the label OUT OF LIMITS, as a separate response category, should be used in

the published distributions. All such cases would then be reported in that category. User's Manual Table 5 (retitled) and its accompanying discussion would then link without ambiguity to a particular category in the distributions. If it seems desirable to distinguish them (and we think it unnecessary), two categories, BELOW LIMITS and ABOVE LIMITS, could be used.

For most items, the category DON'T KNOW (code 94) is misleadingly labeled because it does not reflect an actual response option. For any item where "don't know" was not an available response option in the questionnaire, it offers a falsely precise statement of the number of people who "did not know." We are unclear how RTI determined that the respondent "didn't know" an answer for most items, although a few cases (possibly the result of key punching errors) are so listed for every item. We presume that most such cases were determined either by manual coding of write-in responses or from telephone call-backs.

As we have suggested, much more than relabeling is needed to cope with this coding problem, and we refer back to the earlier discussion for a proposed solution.

## SUMMARY

We reiterate that RTI's coding scheme is, on the whole, well designed for the assessment of certain technical problems, such as item wording, instructions, and format. Since we suppose that the majority of data users will not engage in modifications of the questionnaire, and

might be confused or misled by features of the present scheme, we recommend that any parallel "general use" data tape employ a coding scheme modified along the lines we have suggested.

In our opinion, a general-use data tape probably requires more judgments than RTI permitted itself about the "true state" of the respondent. We think that such judgments can be made on rational and mostly empirical grounds. Our recommendations are offered with a view to altering the coding scheme to incorporate such judgments and to eliminate codes made necessary by the restraints on judgment under which RTI worked.

We stress those suggestions that bear on coding which affects the calculation of LEGITSKIP (the number of respondents not eligible to answer an item). It seems essential that the coding scheme not introduce confusion about the size of the "eligible" pool, since any potential user should have a primary interest in the completeness and representativeness of the available data for an item.

Our suggestions do not modify the "true" situation with respect to item non-response--that is, they do not supply missing data-- but they should make it easier for analysts to assess the adequacy of the data base.

This detailed discussion of problems associated with coding has preceded the discussion of patterns of item non-response to make the reader fully aware of the problematic nature of much of the information on which we have based the analysis of item non-response. No additional caveats are incorporated in the subsequent discussion.

## ITEM NON-RESPONSE

Problems associated with item non-response.

As we have noted, item non-response poses several difficulties for the analyst. Besides biasing data in a manner which is difficult to correct, it may sharply restrict the amount of usable data. An analyst who wishes to examine a number of items concurrently, or to relate responses from several waves of a longitudinal survey, may find this almost impossible.

For example, if he wishes to relate two items, each with a 75 per cent usable response rate, it is quite possible that no more than 50 per cent of the respondents will have answered both items. If more ambitious efforts, say analysis of interaction among four or six items, are to be undertaken, the overlaps may be so poor that the analysis will have to be abandoned or that some method must be employed to "plug" gaps in the data base. Unfortunately, the analyst cannot tell, from the response distributions published in the User's Manual, whether or not his proposed study will be seriously impaired by such problems. Only when analysis is underway (after the data tape has been acquired) will he be able to crosstabulate responses to determine how item non-response affects his analysis plan. *

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* This must occur when single-variable distributions are published. NCES might want to perform, for a fee, skeletal cross-tabulations which would provide prospective users with joint-item response rates.

The shape and variability of response distributions may differ greatly for items with different response rates. Because population estimates drawn from such sample distributions will vary in precision, meaningful comparisons may be impossible. Certain kinds of indexes, such as ratios between average total income and average unearned income, may be precluded.

Biases resulting from general non-response can usually be suppressed by some scheme that applies across all items, such as reweighting the sample. Corrections for item non-response cannot use such schemes, because the items are not independent and because too many separate weights would be required. Some other adjustment scheme must be used to preserve the utility of the data.

Patterns of item non-response.

Procedure. We have analyzed non-response by examining patterns of high and low rates as they appear in the data. Our main focus is on patterning associated with specific types of information, although we have also considered item sequencing and other format characteristics.

We examined a subset of items selected because of their central importance for policy inquiries. These are listed, by number within response levels, in Table 2.* We chose those items that supply

* They are also listed, by straight numerical sequence but without brief content synopses, in Table 3.

information about respondents' current (1973) activities and statuses (e.g., marital status), school and training enrollment and costs, sources of funds for college or other schooling, work experience, income and sources of income, reasons for various acts and decisions, and certain personal and social characteristics (some acquired from about 4,500 respondents only through the First Follow-up Questionnaire, Form B). * Counting all subitems (items which are part of a set of related items) as separate entities, we calculated response rates for 204 distributions published in the RTI User's Manual. The number of items omitted is relatively small.

The items examined constitute the core data sources of the first follow-up survey. They cover all of the matters most likely to concern policy analysts charged with assessing the past or future roles of the Federal government in secondary and postsecondary education. They will be the source of information, for example, for such critical issue areas as the transition from school to work, access to and persistence in postsecondary education and training, and (from future survey waves) economic and other returns to education.

Excepting item F64 (Since leaving high school, have you served in the Armed Forces, . . .), which we excluded along with all other items

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* In the order mentioned, see for example items F1A-F, F7A, F21-F23, F25, F29A, F39, F46, F47, F48A, F54A, F11, F24, F29B, F31, and F78-83. This list does not include every item examined.

on military service, * we examined all of the "key" items listed by RTI in User's Manual Appendix B.

RTI used key items to assess the acceptability of a questionnaire. If answers to any were absent, the respondent was contacted by telephone to resolve problems; if answers to all the key items were present, the questionnaire was accepted and appropriately coded and punched.

An important digression is in order here. RTI's key items omit some which we think crucial, particularly items 46 and 47 (school finances), item F11 (1973 income), and all of the "background characteristics," except race, in items 86 through 99 of Form B--the only source of direct information on these matters for about 4,500 respondents. We think the "key item" check list should have been more comprehensive, and that the key items in future waves should emphasize information crucial to Federal policy concerns. Because educational financing and equal access are central to Federal policies on higher education, we urge that "key items" for future waves of the survey include pertinent items such as those we have mentioned.

To return to our procedure:

We focused on usable response rates, by which is meant the proportion of responses, out of all ruled eligible to answer a given item,

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* Because very few respondents had been involved in the military.

that can be interpreted without ambiguity. This definition implicitly incorporates as "non-response" all cases in which an expected answer was omitted and those for which an answer that was given cannot be used in analyses, i.e., cases bearing "garbage" codes and those bearing routing-error codes.

Cases with routing-error codes were excluded from usable response because they indicate doubt about the accuracy of routing-item responses. Had comparable codes been used to "flag" suspect responses to conditional items--as we have argued they should have been--we would also have excluded those responses. Under the present circumstances, however, this was a practical impossibility. Therefore, the calculated "usable" response rates for conditional items include some responses whose accuracy is in doubt.

We recognize that the inclusion of dubious responses to conditional items inflates the calculated usable response rates. Since we--like any prospective user relying on the published distributions--were unable to distinguish the suspect conditional responses from the others, we were forced to assume that all items were about equally affected by them. While we doubt that this is empirically true, we made the assumption in order to carry out the analysis of response patterning.

For our analysis, we graphed the usable response rate in a strip graph with items ordered according to their numerical sequence in the questionnaire. Some of our comments follow from inspection of

this graph. Their grounding may not be obvious from the same data presented in other formats, but the graph is omitted from this paper because it is unwieldy. An interested reader can reproduce the graph in strip form from the data given in Table 3 (appended).

Statements based on our analysis of response patterns are to be regarded as diagnostic hypotheses, or plausible explanations based on conjecture. We emphasize that recommendations based upon these hypotheses need field testing before they are implemented for collection of basic NLS data.

Varieties of patterning.

Instrument length and format. Decreasing rates of usable item response, referred to here as attenuation, are to be expected in any self-administered questionnaire, as respondents tend to tire of answering questions. Some will simply quit, at some point, if there is no external impetus to complete. The complexity of the routing patterns in the NLS-HS instrument makes assessment of attenuation difficult, since there are several possible beginning-end sequences.

It is possible, however, to compare response rates for items at the beginning of questionnaire Section A with those for the background-data items in Section E, both of which all respondents were supposed to answer. The highest response rates in the initial portion of the questionnaire run in the vicinity of 95 per cent, whereas those

for background data items in Section E run about 90 per cent.* This suggests that attenuation attributable to the length of the questionnaire may be no more than 5 per cent. This is relatively small for such a long and complex self-administered questionnaire, and leads us to wonder whether respondents answered questions in the sequence they were asked.

The sheer length of the NLS instrument seems to exert surprisingly little effect on item non-response. Therefore, we attribute most of it to the questionnaire's complexity, especially its complex routing patterns.

One possible reasons for routing-pattern errors may be that some respondents answered the questionnaire from back to front or by skipping around among sections in the instrument. There was nothing to prevent this pattern of response (as there is in a personal interview). Since we may presume most respondents knew the most efficient mode of multiple-choice test-taking (leaving the hardest questions till the end), it is reasonable to suppose that many followed the same practice in completing the questionnaire. No amount of care in the construction of routing patterns, so long as they assume Item 1-to-Item N sequencing of answers, will wholly eliminate skip pattern errors and their attendant

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* For Section A, we include items 1A, 4, 5, 6A, 6B, 9, 10, 12, 16A. For Section E, items 78A, 78B, 79, 80A, 80B, 80C, 81. The mean usable response rate for Section A items is 93.7; for Section E, 90.1.

loss of usable response. Some formats, however, may be less vulnerable than others to respondents' carelessness.

One alternative approach to routing would make use of special blocking, variations in type style, physical alignment of response options, and the like to direct attention toward the proper item sequence. Figure 1 illustrates, for one troublesome sequence, how physical layout changes might be employed to reduce skip errors.

The layout of the First Follow-up instrument was evidently designed to facilitate editing, coding, and other processing. This purpose is not necessarily compatible with that of directing the user (respondent) through the proper item sequences. Figure 1 is laid out for the convenience of the user, and will probably prove less convenient for direct keypunching, since items and response options are "jumbled" from the standpoint of keypunching.

We may note that use of the form of layout suggested in Figure 1 would probably require printing the instrument across the eleven-inch axis of each page. We think this not only necessary but highly advantageous, since in that format the most convenient way to read the questionnaire is probably from page one to page N. (The inconvenience of other sequences can be checked by attempting to read any booklet containing tables printed across the long axis. It is not impossible to read "out of order," but troublesome.)

The layout format of Figure 1 attacks two sources of skip

23. Any schooling since high school?

Yes_____1

NO_____2

24. Reasons why not _____

| | Yes | No |
|---|---|---|
| a. | 1 | 2 |
| b. | 1 | 2 |
| c. | 1 | 2 |
| d. | 1 | 2 |
| e. | 1 | 2 |
| f. | 1 | 2 |
| g. | 1 | 2 |
| h. | 1 | 2 |
| i. | 1 | 2 |
| j. | 1 | 2 |

Do not answer any other questions on this page.

GO DIRECTLY TO Sec. C, p. 15

25. In school 10/73?

Yes_____1

NO_____2

GO DIRECTLY TO Q. 29A, p. 9

Do not answer any other questions on this page.

26A. What school?

Name:_____

City:_____ State:_____

26B. What kind school?

A. Voc. or trade B. Two-year college C. Four-year college or university D. Other___

26C. Public or private?

Public_____1 Private_____2

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Fig. 1.--Possible layout for routing pattern, items F23 to F26 (schematic).

error. It discourages random entry, owing to clumsiness imposed by the 90-degree rotation. It also makes clear that different choices of sequence flow from particular responses to the routing items: alternative sequences are placed side-by-side, to indicate either-or decisions, much as a fork in the road requires an exclusive choice of path. Lines drawn around exit blocks serve to set the pathways apart visually, and the instruction GO DIRECTLY TO, instead of SKIP, directs attention to the item destination rather than to the pathway excluded.

Use of different type styles for routing-item responses likewise alerts the reader that something is different about the alternative choices. The arrows (which flow directly from the response word--rather than code--to the appropriate item number or direction) should serve to provide "closure" for the implicit question "why are the type styles different?" Use of red, stylized arrowheads* serves further to direct attention to the fact that the respondent is expected to go somewhere other than down the page to the next item, and the juxtaposed item destination tells where.

Physical layout devices like these would, we think, serve to make the questionnaire better suited to self-administration than is the original layout.

Still another possible approach would be to adopt a "tax-return" format, where whole series of conditional items would be placed on separate sheets or "schedules."



* Shown as ▶ in Fig. 1.

With this approach, those items which every respondent is expected to answer would be consolidated in a basic booklet. Depending on his response to each screening (routing) item in this basic form, the respondent would then be instructed to complete specified schedules. Each supplementary schedule would contain only those items appropriate to the screening response, and would bear a prominent instruction to "complete this schedule only if you said ___ to Question X." General instructions for the whole instrument would emphasize that the respondent will not need to complete every schedule enclosed and that he should pay careful attention to the instruction on each schedule.

This format has the disadvantage that the respondent may lose, or choose to ignore, whole blocks of items. It may risk greater information loss than more ordinary "routed" sequences, but "routed" formats do not prevent the respondent from skipping whole blocks, so the new difference in risk may be small.

The "tax form" format seems likely to offer several advantages. First, it should reduce errors caused by random entry. Second, it may encourage overall response by demonstrating that not every question will have to be answered. Third, should there be missing information, it would be possible to mail out only the appropriate schedule, rather than a whole new questionnaire, for follow-up.* Fourth, by appropriate

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* With an appropriate cover letter and special instructions.

omission of schedules, it would be possible to "tailor" instruments to the experience of the respondent as known from earlier waves of the questionnaire, perhaps thereby inducing a higher general response rate as the longitudinal survey progresses.* Finally, it would be possible to request some detailed information only from subsamples, since schedules requesting details could be sent to only (say) 20 per cent of all respondents. This, too, might enhance general response rates.

As noted, we suspect part of the routing errors, to which we attribute much of the item non-response, occur because respondents do not proceed through the questionnaire from the first to the last question. If so, it is imperative to use some format that, as nearly as possible, eliminates the dependence of routing on following a particular item sequence or (failing that) to make the correct sequence so obvious that it is hard not to follow it. Our formatting ideas attempt to achieve this.

Attenuation within blocks. A second major pattern involves response attenuation within sets or blocks of items. There appear to be several different sources for this.

The first seems to depend on the probability that an offered response option applies. Items F1 and F16** illustrate that matter as

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* This notion is predicated on the assumption that response is more likely if some evidence is given that previous information is actually being used in some way. An instrument "package" that shows both concern for the respondent's time and awareness of previous responses should help in this regard.

** Present (1973) activity and anticipated (1974) activity, respectively.

shown in Figure 2. The relationship shown there is imperfect, but it seems evident that response rates drop as the probability (percentage "applies to me") of the activity decreases. In these examples, response above some "base" rate (here, about 85 per cent for specified activities) depends on the proportion of people to whom the item applies.

Item F47 (sources of funds for schooling) provides a more dramatic example. In the published distributions, it is treated as seven pairs of subitems with a fund source and amount as each pair. Usable response rates range from about 65 per cent for the "first-listed source" to near zero per cent for the "seventh source." Obviously, the explanation is that the number of people having seven separate sources of funds (that they can report) is far fewer than the number having one. The drop-off in response between the "first" and "seventh" sources forms a nearly uniform rate of attenuation within the block.

Attenuation of this form can probably be attributed to respondents' reliance on non-response to express "not true in my case" or some similar meaning. We have remarked earlier on the need for greater use of "don't know" and "doesn't apply" response options to reduce non-response of this type. In the case of item F1, however, "does not apply to me" is offered, and the effect still occurs. Perhaps a simple YES and NO option pair would have been better.

A modified form of the same pattern appears for three sets of items asking reasons for decisions about postsecondary schooling

| ACTIVITY | F1: Present "Applies"[a] | F1: Present Response Rate | F16: Anticipated "Applies"[a] | F16: Anticipated Response Rate |
|---|---|---|---|---|
| Working | 64.3% | 94.6% | 73.6% | 92.8% |
| Academic Course | 41.5 | 90.0 | 48.2 | 88.9 |
| Homemaker | 16.0 | 86.2 | 21.4 | 84.8 |
| Vocational Course | 14.2 | 87.3 | 20.0 | 85.2 |
| Military | 5.4 | 86.6 | 6.2 | 84.4 |
| "Other" | 5.7 | 65.7 | 6.0 | 63.5 |

[a] Response "applies to me" or "expect to be doing" as percentage of usable responses (each subitem).





Fig. 2.--Correspondence between Activity Probabilities and Item Response Rate.

(items F24, F29B, and F31). Although the response rates are quite uniform for the several subitems (individual reasons) within each set, the average rate drops by about 25 per cent between F24 and F29B and by a like amount between F29B and F31. Each successive item (set) applies to a smaller proportion of the sample; given inflation of eligible pools, the constant decline in response rates probably reflects the noted tendency to use non-response as a way of expressing "doesn't apply."

Items F24 and F29B probably seemed identical to some respondents. Item F24 requests reasons for not continuing formal education "after leaving high school," and F29B is identically worded except that it stipulates "right after leaving high school" (emphasis added). We think it likely that some of the decline in average response (to the set of reasons) stemmed from this seeming duplication. We consider "seeming redundancy" a second source of within-block attenuation.

Both the "probability" and the "redundancy" effects are allied to within-block attenuation based on increasing detail. Consider, for example, item F11, requesting information about the respondent's and his/her spouse's 1973 income. For some, TOTAL INCOME and INCOME FROM WAGES, SALARIES, (etc.) may seem redundant. For most, income from "other" sources listed--interest, rental property, public assistance, unemployment compensation--will not apply.

Response rates drop sharply, for both respondent and spouse, over the several subitems in F11. Both improbability and seeming

redundancy probably contribute to response attenuation in this case. *

Even when "probability" and "redundancy" are less important influences, requests for details are resisted by non-response. For example, there are several instances where usable response rate for routing items (with error-coded cases excluded from usable response) are substantially higher than the rates for "detail" conditional items (with some "error" responses included as usable). ** All of the critical routing items exhibit this pattern, as shown in Table 3. There, usable response rates fall between items:

- F23 and F24 (any postsecondary schooling and reasons why not),
- F25 and F26-F27 (enrolled as of 10/73 and various details about the school in which enrolled),
- F29A and F29B (enrolled 10/72 and reasons why not),
- F30 and F31 (same school in 10/72 as in 10/73 and reasons for changing),
- F48A and F48B-F50 (any job in 10/73 and details about why not or what kind of job, wages, hours), and

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* Similar influences probably operate for items F82C - F82DC and F83C - F83DC (application for financial aid and amounts received, first- and second-choice colleges).

** This pattern may be an artifact of the inclusion of the 40-coded respondents in the eligible pool for conditional items. Data in Table 1 suggest, however, that the drop in response rates is generally too large to be accounted for by the inclusion of the 40-coded respondents. In addition, their influence on the conditional items is offset to a degree by inclusion of the 20- and 60-coded respondents, who contribute to "usable" response (excepting PARTIAL RESPONSE codes).

- F54A and F54B-F56 (any job 10/72 and details about why not or type of job, wages, hours).

Inspection of the response rates for these examples suggests, not surprisingly, that respondents simply resist answering questions that ask for a lot of detail.

Since much of the item non-response occurs within blocks, our examination leads us to the quite conventional conclusion that item non-response for the instrument as a whole would be reduced if fewer details were requested and if unlikely events were excluded. In short, a simpler and more generally "relevant" set of questions would reduce the problem of item non-response.

However, elimination of detail might defeat the purpose of data collection. If details cannot be deleted and the purpose still met, it may be useful to modify the ordering of requests for detail. The patterns described above suggest that if unlikely options and/or "fine line" detail were requested first in a sequence, item response might be somewhat enhanced.

It will not always be possible to follow that format. In some cases, the biggest drop in response rates occurs between a routing item and a set of conditional questions; there, the order certainly cannot be reversed. If, however, conditional items were to appear in a separate "schedule," as suggested above, some modification in the ordering of the details might be possible. For example, the longer time lapse

between response to the routing item and completion of the details might give the respondent a long enough pause to permit him to search his memory or his records for the requested information, without interrupting an otherwise smooth task flow. *

Where non-response is based on a respondent's lack of information, no formatting tricks are likely to affect non-response appreciably. But a combination of easy-to-follow format, reduction of vulnerability to random points of entry, addition of "don't know" (or similar) options among the precoded responses, elimination of non-essential detail, and exclusion of options that are likely to apply to very few respondents (or, better use of "does not apply" as an option) should serve to enhance item (and possibly general) response.

We again emphasize that these suggestions are not to be implemented without careful field tests. A field test of various alternatives, using identical questions but varying the questionnaire format, should indicate whether the suggestions will work to increase item response.

We remarked in the opening pages that changes in formatting and response options might cause noncomparability between waves of the survey. From a "purist" standpoint, this will certainly be true. From a practical standpoint, some changes may be so minor as to cause little

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* Instructions might call for completion of detail schedules after completion of the basic form. By thus controlling the task flow, interruptions might be "scheduled" remotely.

concern, but others may be very influential.

We have suggested format changes because we think future surveys will yield very little useful information without them. The combination of sample attrition and item non-response rates as high as many in the first follow-up could so far reduce the available data for some items as to make generalizations to the original population impossible.

It may be necessary to choose between data with doubtful comparability and no useful data. Given the resources already expended on the NLS-HS project, the wiser course seems to be acceptance of some non-comparability. Future instruments must be designed not only to remedy the problems we have discussed but also to minimize non-comparability resulting from changes. Whether these objectives are compatible can only be determined from experience. Hence, our repeated emphasis on field testing of any instrument modifications.

Kind of information requested. Sources of non-response identified above are all based on quantitative considerations--too many questions in general, too many details, too few people to whom a response option may apply, too few response options, and so forth.

We turn now to patterns which appear to be based on qualitative considerations, i.e., items which seek information of certain kinds which respondents are unwilling or unable to provide.

In Table 2, items are grouped by usable response rate; the content of low-response items differs fairly systematically from that of

the high-response items, which allows us to identify certain kinds of information that are especially likely to be omitted.

First among these is the familiar "money" item. Item non-response tends to be high in any survey not conducted by personal interview* when a request is made for information about incomes, expenditures, savings, and the like. Presumably, this is grounded in part on a cultural prohibition against divulging this kind of information and in part on ignorance of details about personal or family finances. Since norms about the propriety of disclosing financial information may vary among subcultures, item non-response involving this kind of information is not easily prevented by any one stratagem.

As a rule, respondents are more likely to comply with requests for information made personally by an interviewer. Consequently, it is common (though not always effective) survey practice to use telephone or personal call-backs to obtain omitted financial information when the cost of doing so is justified by the expected benefits of the research. We commented above that RTI's list of "key items" for the manual edit excluded certain critical financial items for which non-response should have triggered call-backs, and urged that they not be omitted in the future.

Resistance based on taboos against financial disclosure is only one source of financial omissions. Lack of information, or simple

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*And often in personal-interview surveys as well.

laziness, is probably at least as important and often more easily dealt with. It is sometimes possible to lead respondents, by small increments, into full disclosure. This generally entails a series of very simply worded questions about a long list of alternative sources. In addition, it helps to ask for answers in terms of rather broadly categorized response options rather than asking for a specific number, unaccompanied by suggestions.

The use of supplied, categorized response options will tend to reduce accuracy for those respondents who know quite well, and are willing to report, amounts of money. Hence, it may be necessary to choose between accurate estimates from very few respondents and rough estimates for a greater number of respondents. Since the accuracy of financial detail is generally not high in survey studies, it is probably better to provide "pegged" response options and accept their inherent inaccuracies.

Depending on prior knowledge, it is often possible to establish well-delimited options within a known range. For example, students in the NLS might be supplied options in the range "zero" to "over $2,500" with choices at $500 increments, and given an instruction phrased roughly as: "Mark the amount which comes closest to the amount of your scholarship income during 197-. If you had no scholarship, mark 'does not apply.' If your scholarship was less than $250, mark zero; if it was more than $2,750, mark 'over $2,500'." Such detailed and simply-worded instructions extend the space requirements of the item, but may increase usable

response.

Supplied options and detailed instructions may be expected to aid recall and to relieve the respondent from the burden of giving an impossibly precise answer. A "don't know" option should also be provided, with instruction to use it only when there was income from the source but the amount cannot be recalled well enough to use the ranges.*

It might also be possible to increase item response by clustering all financial questions in one block. The advantages of this approach are that the respondent can focus his attention on a single type of information, can take time to collect records, and can use one item to aid his memory on another. This last, however, may be an important disadvantage, because interdependence among several items may transmit errors throughout the entire block and because there may be erroneous transference among the items (halo effect). Another disadvantage is that a respondent may omit all financial information when thus blocked, whereas he might supply at least some part when items are separated. Clearly, such a scheme requires empirical field testing before being adopted for actual data collection.

Non-response on financial items is doubtless the most serious concern (because the data are essential and potential remedies are few and may introduce new distortions), but the second prominent type of

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* The use of "income" in these passages is for illustration; the same stratagem may be useful for other financial data.

information lost presents almost equal difficulties. In this category are items which request information about the respondent's reasons for doing or not doing something.

As we have said, we suspect part of the high item non-response here either stems from respondents' use of omission to express "no" (or, "does not apply") or is an artifact of the routing-error or other codes applied. In part, as well, it may be that when questions appear redundant, as discussed above for the case of F24 and F29B, respondents believe they have already answered the question and see no need to repeat themselves.

Response rates for "reasons" blocks are lower than 80 per cent, and more typically lower than 60 per cent, for every such item we examined, neither of which is true for all financial information. We doubt that these low rates result wholly from the questionnaire design. As a hypothesis, we suggest instead that the explanation may lie partly in the fact that the sample consists of people leaving adolescence and entering adulthood. This may be important because at that time personal autonomy is a strong motive, and protection of newly-won or sought-after adult rights is a major consideration. Acts and decisions may be made without strong reasons other than the assertion of claims to adult status and, once made, must be defended against adult criticisms. Under such conditions, requests for "reasons why you did X" might be viewed as a call for justification, to which the emerging adult may respond negatively.

For "reasons" items, as for all others in the questionnaire,

it must be remembered that the wording of the items carries the load of "rapport" which, in a personal interview, is carried by the interviewer's manner and expression. The questions must convince the respondent that the information is really necessary, explain in very simple terms just what to do for any conceivable respondent situation, and express recognition that the respondent is doing the researcher a favor by taking time to answer the item.

Wording of items is, therefore, more crucial for a mailout questionnaire than it is when personal interviews are to be conducted. The researcher must depend on "cold" written language to perform all the persuasion and appreciation-giving tasks which are otherwise the responsibility of an interviewer. To accomplish this, it may be necessary to use longer, more emotive items than would be used with personal interviews. The need for such language may be especially great for the NLS-HS sample, for reasons like those noted.

For "reasons" items, more deferential question wording might enhance response. The present item F24 ("Here are some reasons others have given for NOT continuing their formal education after leaving high school. Which of these reasons, if any, apply to you?") offers a set of reasons acceptable to adults and demands that the respondent claim one of these as his own. It might be better to put the question in language suggesting belief in the autonomy, uniqueness, and privacy rights of the respondent. Perhaps something like: "There are many

possible reasons why a person does not continue formal education after high school. Your reasons may be among those listed below. Please circle YES for those that apply in your case, and circle NO for those that definitely do not. If your reasons are not listed, circle NO for all those listed, circle YES for "OTHER," and give a brief description of your reasons in the space provided." While such wording makes the item longer, it uses simple and deferential phrasing, gives an instruction for what to do in any case, and, by the inclusion of a free-response choice, allows the respondent to maintain his belief in his own uniqueness even though the chances are that he will choose one or more of the listed reasons.

It may be that high non-response to items asking for reasons chiefly reflects the post-adolescent status of these respondents. If this is correct, response rates on "reasons" items should rise over time, simply as a function of the respondents' growing self-assurance and belief in the security of their adult status.

A third kind of information commonly omitted is that which is not likely to be readily recalled by the respondent at the time he completes the questionnaire. In Table 2, it can be seen that response rates tend to be low for items seeking information about time-remote events (either past or future), details, or about other people or organizations external to the personal experience of the respondent. Individuals in the age group to which the survey is addressed are typically in a period of

transition between roles and social statuses, a time when concerns about the present and about oneself may be most important. Under such circumstances, requests for information about the past or future, about others, and about details demand facts that may not be "up front" in the respondent's memory and which he may even think trivial. If such information is essential, members of this sample will probably have to be lead into responding by aids to recall and deferential encouragement.

Summary.

A critique drawn from hindsight is, of course, easier to produce than a foolproof instrument designed before benefit of field experience with the specific sample in use. Our comments are not intended to denigrate either the instrument or its designers, and we urge that they not be so taken. Rather, we hope that the experience gained from the first follow-up can serve as a basis for improving item response in the future.

Our strongest recommendation is that future questionnaires be designed with greater consideration for the intended "audience" and the mode of data collection. The instrument must be made to do the guiding and "rapport building" work of an interviewer. The weakness of the instrument for use with self-administration is well documented by RTI's comment about manual edit failures:

> Approximately one-third of all mail-returned questionnaires failed manual pre-machine edit and required telephone follow-up to some degree; less than 5 per cent of all questionnaires resulting from individual interviews by Bureau of Census personnel failed this edit. (User's Manual, p. 21)

It seems probable, given usable response rates ranging from the low 70s to low 40s for various parts of items F46 and F47 (first year school costs, fund sources, and amounts) that manual edit experience with the mailed returns would have been still worse had these items been among the "key" items.

Even with the manual edit and the call-back procedure, unambiguous usable responses to item F23 (any postsecondary schooling or training) were a woefully small proportion (87.5 per cent) of the answers to this critical item. One wonders about the adequacy of telephone follow-ups which permit about 12 per cent "error-coded" responses to perhaps the most important routing item in the entire questionnaire. RTI says:

> Questionnaires which failed the manual edit process, due to having insufficient information on the "key" questions, were examined carefully by telephone follow-up staff in preparation for a telephone interview with the respondent. Telephone follow-up operators were trained . . . so that they would be capable of coping with any questionnaire-related questions a respondent might bring up. (User's Manual, p. 21)

Evidently, either the questionnaire was so complicated that the trained operators couldn't prevent 12 per cent routing-pattern errors for item F23, or else their level of performance was quite low. Since the telephone call-backs focused on rather few items, we think it is probably the questionnaire's complexity (rather than the operators' performance) that accounts for the experience with F23.

Our second urgent recommendation is revision of the response options and coding categories, to encourage use of "don't know" or "not

applicable" instead of item omission and to simplify data processing and analysis. We have discussed this in such detail that no further comment is needed here.

Third, to combat routing-pattern errors, we have suggested that the physical format of the questionnaire be changed. We urge that it be assumed respondents may answer questions in any order of their choice and may not complete the questionnaire at one sitting. These assumptions require that routing be made, as nearly as possible, independent of item sequence. Our formatting suggestions rest on this requirement.

Fourth, we have suggested that much of the information sought may be deemed trivial by the respondents, or be outside the scope of their everyday memory. Admonitions to "think carefully" probably will not suffice, especially in the context of a long and rather detailed questionnaire. They must be replaced or supplemented by language and format which leads the respondent into the areas about which information is desired and which permits him to state that he just doesn't know, with no implicit stigmatization for making that response.

In addition to these suggestions, we think there should be a frank admission (in the general appeal prefacing the instrument) that the information sought may seem trivial to the respondent but is nonetheless important to policy makers. This should serve to prevent some item nonresponse.

Finally, we urge that no information be sought which is not in fact essential for policy-making purposes. We presume that all of the items in the first follow-up questionnaire were screened and justified on such grounds. Nevertheless, in a period when general resistance to survey research and to "government prying" are prominent, extra precautions must be taken to assure that no unessential questions are asked.

# ASSIGNMENT OF DATA TO ADJUST FOR ITEM NON-RESPONSE: ISSUES, METHODS, AND EXPERIMENTATION

## Introduction.

Our review of several approaches to assigning data as a means of compensating for item non-response reveals little consistency of practice and a paucity of information pertaining to the matter. A search of listings in a key Census publication yielded no articles relevant to our present discussion.* Both the inconsistency of practice and the dearth of literature suggest that the problem has not been given the methodological attention it deserves.**

The discussion following covers a wide range of topics. We begin with some general concerns about the wisdom of making data assignments, continue with critiques of several approaches to the problem of item non-response, and end with a discussion of methodological matters, including a suggestion for isolating appropriate values which might be used when assignment is essential.

We argue below that data assignment is to be preferred over

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* Bureau of the Census, Indexes to Survey Methodology Literature, Technical Paper No. 34. Washington: GPO, 1974. Certain recent articles on the "hot deck" and other procedures were brought to our attention, but we have excluded the methods they discuss for reasons stated in the main text.

** Blau and Duncan (1967), summarized below, provide an excellent discussion of influences of item non-response on correlations between one pair of variables.

deleting respondents with missing data because it allows the analyst to keep the sample representative. To the argument that assigned values should not be used for longitudinal analyses, we reply that a carefully chosen assigned value is better than none at all if the item non-response is high.

We offer two major recommendations. First, we suggest NCES undertake a series of empirical investigations aimed at assessing the effects of various methods of data assignment upon fundamental characteristics of the data, such as the shape of distributions, variability, measures of central tendency, measures of change, and measures of intertemporal correlation. Unless such investigations are conducted, the effects of any method can only be assessed speculatively. We think the long-range utility of the NLS-HS data base justifies the time and expense of such investigations.

Second, we suggest that NCES use the information from such studies as the basis for establishing its own policies for in-house analysis, and for preparing a manual on data assignment to be circulated among prospective users of the NLS-HS data. It seems to us that the best way to please all prospective analysts is the publication of a manual, to be used by the analyst in making his own decisions about data assignment.

NCES should also consider the merits of including assigned data in the parallel analysis tape suggested in the previous sections of this paper. We think that prospective users might be given an option

between data files with and without assigned data.

Basic issues: an overview.

There are two issues which must be resolved: (1) whether to attempt assigning values and (2) how such values may be assigned. The decision as to whether imputations should be made is not entirely a technical matter, and probably should be left to each research user of the NLS-HS data, who can consider his own research objectives and the consequences of data assignment for them.

The chief advantage of missing-value assignment is that it helps maintain the size of the data base available for analysis and the representativeness of the sample. The chief disadvantage is that the enhanced data base may lull the unwary into feeling that the data are more complete or precise than in fact they are and, therefore, placing too much confidence in the results of analysis. In general, where static description of a population is the goal, judicious assignment of missing values may improve estimates of distributions. Assignments may become dangerous, however, when the objective is estimation of sequential or causal connections between events or states (e.g., analysis of dynamic processes).

The most common approach to missing-data adjustment involves assigning some category (or, more accurately, subcategory) mean or median value to an individual missing case.* The hazard

* Of those examples we review below, only the approach taken by the Bureau of the Census departs from this practice; there the objective is to duplicate a known distribution rather than to establish a data base for examining "causal" relationships.

involved in assigning subgroup means or medians to individual cases, for longitudinal analysis, is that such a practice will certainly reduce variability within any subgroup of cases containing assigned values and is likely to reduce correlations of sequenced events (states).

A more subtle problem involves compounding of assignments: an assigned value might become part of the basis for assigning another value to an individual. In the event that analyses involve examining relationships between two variables, one could end by relating a variable to itself (i.e., to a composite heavily loaded on the original assigned value). Such a feat would, clearly, exaggerate estimated relationships in proportion to the number of dual-assignment cases* included in the analysis and the extent to which the first assigned value determines the second.

On the other hand, failure to assign missing values may also distort analysis. If item non-response is systematic, both descriptions of the population and estimates of correlation may be misleading. For example, suppose that people with high levels of educational attainment, but relative low incomes, omit their incomes (perhaps to "save face"). Suppose further that people with little education, but relatively high incomes (e.g., from illegal sources), likewise omit their incomes. Given such a systematic pattern of item non-response, the correlation between

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* That is, the number of cases for which "both" variables carry assigned values.

education and income would be artificially inflated if no estimates of income were assigned. Assignment of missing values under such circumstances would not guarantee accurate estimates of the degree of association between variables; some assignment techniques described below, however, may help avoid the problem of exaggerated correlation.

Since distortion can occur with or without assignment of missing values, no general statement about the wisdom of assignment can be made. The matter must be left to each researcher, who will take responsibility for safeguarding his analyses from the particular kinds of distortion least acceptable within the framework of his problem.

For these reasons, we recommend that NCES not attempt to provide only tapes with data augmented by assignment of missing values. NCES might, however, choose to offer two versions of NLS data tapes, one without assignments, one with assignments appropriately coded as such, i.e., the parallel analysis tape discussed previously. A better approach to the problem is for NCES to provide data users with a technical manual containing detailed discussions of various possible "fixes" for item nonresponse. The manual should include instructions for carrying out those procedures deemed most suitable for different uses of the data base and/or citations of sources for such detailed instructions.

In its own analyses, of course, NCES may wish to adopt some standard policy on assignment of missing values, so that comparability among its various studies will be maintained (and to forestall erratic

treatment of the problem which might result from personnel turnover within NCES). NCES staff analysts or their contractors should be free to decide whether or not assignments should be made in the context of their particular studies, but procedures for making assignments should be standardized. It may not always make sense to assign values for missing cases but, if it does, the procedure should be uniform from study to study. Our second recommendation is that analysts working with the NLS-HS data under NCES auspices should be given discretion with regard to whether or not data are to be assigned, but little discretion as to how assignments are to be made. *

Both of the foregoing recommendations assume the existence of knowledge about the effects of various methods of assignment upon the results of analyses performed with the NLS-HS data. Since it is not clear that adequate knowledge presently exists, our third recommendation is that NCES undertake empirical studies aimed at examining the consequences of applying any assignment procedure to the NLS-HS data. Any decisions about standardization of NCES procedures, or any recommendations included in a data users' manual, should be based on rather extensive experimentation with the data base itself, since either kind of judgment is likely to--and should be intended to--stand for some relatively long period. Decisions based on theory alone or on experience with

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* Perhaps a set of limited options could be provided, permitting some discretion as to choice of method.

assignment procedures used on other data bases may prove inadequate for NCES's in-house standardization or for a users' manual.

Assignment practices: review and critique.

The following discussion focuses on general approaches to the problem of item non-response.

Procedure. Our discussions of assignment procedures are limited to general descriptions, sufficient to provide a basis for considering possible consequences of each approach; details are available from the sources cited. The discussion focuses on longitudinal, rather than cross-sectional, analyses of the data because the primary purpose of the surveys of 1972 high school seniors is longitudinal analysis. We are more concerned, for example, with the effects of data assignment on correlation of individual values over time than on associations among subcategories of respondents.*

The discussion is based on an examination of approaches used in several large-scale surveys, all but one sponsored by the federal government and all involving samples intended to represent major segments of the U.S. population. Four of the surveys employ panel designs; three of these are ambitious efforts to follow panel members for several years. The analysts whose approaches are discussed have, in the main,

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* The longitudinal orientation does not, of course, exclude concern for the effects of assignment upon distributions. Trend analyses as well as panel analyses may be considered longitudinal by some methodologists, and it is not our intention to preclude such a definition.

held concerns about data assignment like those faced by prospective analysts of the NLS-HS surveys.

Needless to say, the studies discussed employed machine processing* of large volumes of data, and most have engaged the efforts of many analysts. It may be supposed that the approaches used were based on well-informed judgments, made by many qualified professionals, about the relative merits of available treatments.

While the number of surveys discussed is small, those examined represent major efforts with a degree of complexity comparable to the NLS-HS survey(s), and may, therefore, be considered a reasonable judgmental sample of such surveys. This "sample" includes Project SCOPE, Project TALENT, the Educational Opportunity Survey (Coleman Report and subsequent Office of Education analyses), the OEO-University of Michigan Panel Study of Income Dynamics, the DOL-Ohio State University National Longitudinal Surveys of Labor Market Experience, and the 1970 U.S. Census of Population.

Varying approaches. Our examination of these survey studies shows that no single approach has been accepted for general application. The analysts of Project TALENT, the Coleman Report, and the DOL-OSU labor market surveys make no assignments (some give reasons,

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* An important qualification because machine processing is involved in subsequent considerations of advantages and disadvantages of the various methods. Special coding may be required in some instances to forestall certain "errors" which can arise in machine processing. On the other hand, some methods are feasible options only with access to appropriate computer programs.

others appear to ignore the matter), while analysts for Project SCOPE, the OEO-UOM income dynamics study, the OE analysts of the EOS survey, and the Census statisticians each use a different method of assignment. Excepting the Census, which uses the "hot deck" (a random match) procedure, adjustments are always made by some variation of assigning a mean (or median) value. The variations lie in what mean is assigned and/or how the subcategory whose mean is to be assigned is chosen.

Project TALENT.

Sources: (1) The Project TALENT Data Bank: A Handbook (J. G. Claudy, ed.); Palo Alto, Calif.: American Institutes for Research, 1972

(2) Flanagan, J. C., M. F. Shaycroft, J. M. Richards, Jr., and J. G. Claudy Five Years After High School; Palo Alto, Calif.: American Institutes for Research and the University of Pittsburgh, 1971

The TALENT staff seems to have ignored the problem of item non-response, though this is something of an exaggeration. The handbook (1) makes a brief mention of the problem:

> A potential problem with any Data Bank study is that of missing data. A great deal of information was collected on each participant in 1960 and virtually every case is missing a few data items. In correlation and related types of analyses these missing data can seriously affect the results. There are several ways that researchers can handle this problem: (1) completely eliminate from the study any case with missing data on the variable of interest; (2) base the individual summary statistics on all cases for whom the variables of

> interest are available (e.g., use a missing data correlation program); (3) substitute the sample or population mean, median or some other value for the missing value. The researcher also has the option, of course, of specifying some other procedure. (1:21)

For their own work, the TALENT staff appears to depend chiefly on the deletion of missing-data cases. Discussing the computation of the "Socioeconomic Index" (a key control variable drawn from nine Student Information Blank items), the handbook says:

> Items to which a student gave a non-applicable response were not included in the computation of his . . . socioeconomic index . . . Each student's response to each of these /SIB/ items (excluding those items which he omitted or to which he gave a "not applicable" response) were converted . . . to standard scores /which were subsequently/ used to compute his /socioeconomic index/ score . . . (1:46-49 passim.)

Clearly, no assignments were made for missing data in the computation of this critical control variable.

In the research reports presented in (2), there appear to be two distinctive approaches:

a. For descriptive studies, tabulations include a residual category containing all non-specific cases (e.g., "don't know" plus item non-response);

b. For studies of correlation, the data base is simply the cases for which appropriate data are available.

Tabulations (including percentages) are generally based on data weighted to represent the 1960 high school population. Correlational analyses tend to use unweighted data, unadjusted for item non-response. These

practices follow the dictum given by Shaycroft and Richards:

> . . . assignment of appropriate weights is a crucial step in data analyses the results of which are supposed to be accurate estimates of numbers of cases or percentages of cases in specified categories in the corresponding segment of the national population . . . For many other kinds of analyses, where what is sought is relational data, and the answers to questions about relationships between various variables, weighting cases differentially is of far less importance and in some cases probably quite undesirable. Correlation matrices are an example of kinds of data analysis in which the use of unweighted data is generally quite satisfactory. (2:1-15)*

Most of the correlational studies reported in (2) drop missing cases from the data base, in accord with the advice given by Shaycroft and Richards and the second option noted in the handbook (1).

We question both the advice and the practice. First, we know of no statistical reasons why weighted data should not be employed in correlational analyses.** Second, it should be recognized that dropping missing data cases in effect weights the data, because some members of the sample are "assigned" a weight of zero. Thus, TALENT analysts

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* The page numbering system used in (2) opens the possibility of confusion in citations. Pages are numbered with a digit for the chapter followed by a dash and one or more digits for the page within that chapter. This quotation is, then, to be found on the fifteenth page of chapter one.

** That it can be is indicated by Nie et al., who have included a weighted data correlation subroutine in the Statistical Package for the Social Sciences, and by use of weighted data in the regressions run for the Coleman Report (Coleman et al., 1966:571). They may object on grounds like ours (lack of independence) or because they fear erroneous attribution of statistical significance to correlation coefficients, which might result from inflated sample size.

have used weighted data even while objecting to the practice.

Where it may be possible to do so, it would seem preferable to weight the available cases to compensate for both general and item non-response. To do so would permit the analyst, rather than the non-respondents, to control the representativeness of the sample from which correlation estimates are derived.

Whether or not it is feasible to use weighting to compensate for item non-response is a separate matter. In the most common instance, a substantial amount of missing data comes from people who omit answers for only a few of several variables to be related. This alone would imply a complex effort to assign weights. In addition, the common case implies that any one individual will have answered at least one of the items. This makes weighting nearly impossible, because variables for each individual are not independent. Weights assigned to compensate for missing data on one variable would result in mis-weighting other variables where a response is present. When every missing-data case lacks responses on each variable under study, weighting might be acceptable, but we think this is a rare instance. Even in such instances, the amount of effort required to determine an appropriate weight would probably prove prohibitive.

The whole point of adjusting data for any kind of non-response is to improve the representativeness of a sample, hence the accuracy of population estimates. Faced with varying rates of item non-response, an analyst must decide whether some adjustment he can make will eliminate

systematic biases that result from non-response. If weighting introduces systematic biases, it may be as great a source of distortion in population estimates, for correlation coefficients or other summary measures, as is the non-response for which it is intended to compensate.

The TALENT analysts appear to overlook the self-weighting which results from item non-response. Their only suggested method for adjusting data (assigning the population or sample mean) would result in very crude estimates indeed, and would tend to reduce most correlations. Hence, they prefer to calculate correlations with "unweighted" and unassigned data. For the reasons we have outlined, and because there are now ways of assigning much more refined values, we find the TALENT approach to item non-response not a suitable model for the NLS-HS surveys.*

Educational Opportunity Survey - Coleman Report.

Source: James S. Coleman, et al., Equality of Educational Opportunity. Washington, D. C.: (DHEW-OE) National Center for Educational Statistics, 1966

The Coleman Report approach to item non-response is given by a single statement "buried" in the technical appendix:

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* Their policy of showing item non-response as a separate category in tables is a point in their favor. Unfortunately, they have mingled "don't know" and other residual categories with actual non-response, thereby losing the analytical value of "don't know." Summary measures for such a category will be largely meaningless.

> The estimated totals, averages, and proportions reported in section 2, of the report have been developed by the use of a ratio estimation procedure. This procedure was carried out for each of the five racial composition groups in each of the primary sampling units. These weighted area statistics were then combined so as to produce the desired regional and national estimates . . . No allocations or imputations were made for item non-response. Averages were calculated only on the schools who responded on the item. Proportions were calculated on all schools, with the proportion not responding calculated as a separate category. (Coleman, et al., 1966:558; emphasis added.)

So far as we are able to determine, the underscored statement stands without accompanying justification as the sole comment on treatment of item non-response. Although some attention is given to the problem of respondent reporting error (pp. 568-70), there is no comparable statement on item non-response, hence we must presume no special effort was made to assess potential biases from that quarter.

The technical discussion of the methods for the regression analyses states only that a pairwise-deletion procedure was used in calculation of correlation matrices:

> Missing data was /sic/ treated as follows: correlations were calculated by use of each case for which both variables in the correlation were present. Thus, a case with a missing observation was deleted only for those correlations in which this variable was involved. (Coleman, et al., 1966:571-72)

For constructed variables (indexes) used in the regressions, each item employed in the index was standardized with mean equal to zero. Within this schema, item non-responses were assigned zero, which, as the authors note (p. 572), is equivalent to assigning the

population mean before standardization.

It is worthy of note that the Coleman Report obviously has no fixed policy for treatment of missing data. For descriptive work, no assignments are made and the non-response is treated (or so we are told) as a separate residual category, which is included in the bases for percentage calculation. For correlational analyses, however, two other approaches are taken: pairwise deletion in the case of simple variables included in zero-order correlation matrices, but assignment of the population mean to variables upon which constructed indexes are based.

There is no way of assessing, short of extensive reanalysis of the raw data, how this rather casual treatment of item non-response may have affected the interpretation of numerical results, but one may assume that there was some effect. The total number of assignments is likely to rise when an index constructed from many variables is employed because it is likely that different people will omit given items, causing the number of cases with at least one assignment to rise as the number of variables in the index increases. Consequently, when an indexed variable is correlated with a single variable, assignment of values for item non-respondents is likely to influence the relationship more than it would when two simple variables are correlated. If there were any substantial number of missing-data cases included in the indexed variables, correlations between those variables and simple variables having complete data

would probably be reduced. *

Given these considerations, and the heavy dependence of the Coleman Report on correlational analyses, it would seem that some more consistent policy for treatment of missing data should have been followed. It should be clear that a policy for treatment of item non-response must be decided before analysis begins and must take account of the plan for analysis. The Coleman Report fails on the last criterion. **

National Longitudinal Survey of Labor Market Experience.

Sources: (1) Parnes, H. S., R. C. Miljus, R. S. Spitz and Associates; Career Thresholds; A Longitudinal Study of the Educational and Labor Market Experience of Male Youth, Manpower Research Monograph No. 16 (three volumes). Washington: U.S. Department of Labor, 1970, 1971

(2) Shea, J. R., R. D. Roderick, F. A. Zeller, A. I. Kohen and Associates; Years for Decision: A Longitudinal Study of the Educational and Labor Market Experience of Young Women (Vol. I) Manpower Research Monograph No. 24. Washington: U.S.

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* Unless, of course, variables with assigned values exert no influence on the index value--hardly a likely event.

** In fairness, it must be said that the Coleman Report was a very ambitious project carried out under severe time constraints imposed by the U.S. Congress. Only the most pressing analytical problems could be given close attention, and item non-response may well have been the least of the problems faced by the analysts.

Department of Labor, 1971

(3) The National Longitudinal Surveys Handbook. Columbus, Ohio: Center for Human Resource Research, The Ohio State University, October 1975.

The same general statement regarding assignment of missing data appears in each volume of source (1) and, in modified form, in sources (2) and (3). It may be presumed that the same approach has been taken consistently. The Ohio State group follows a more consistent policy than either the TALENT group or the Coleman Report, in that item non-response is apparently deleted throughout:

> In calculating percentage distributions, cases for which no information was obtained are excluded from the total. This amounts to assuming that those who did not respond to a particular question do not differ in any relevant respect from those who did--a reasonably safe assumption for most variables, especially when the number of no responses is small. (1,vol.1:3-4)

> None of the (questionnaire) edits included an allocation routine which was dependent on averages or random information from outside sources, since such allocated data could not be expected to be consistent with data from subsequent surveys. However, where the answer to a question was obvious from others in the questionnaire, the missing answer was entered on the tape.
>
> . . .
>
> Further, some of the status codes which depend on the answers to a number of different items, were completed using only partial information. The most obvious example is the current employment status of the respondent . . . This is determined by the answers to a number of related questions. However, if one or more of these questions is not completed but the majority are filled and consistent, the status is determined on the basis of the available responses. This gives rise to an artificially low count of "NA's" for certain items. (1, vol.1:211-12)

The justification for non-allocation is a reasonable but weak one. Its weakness lies in the implicit assumption that "no data" will be more consistent with data from subsequent surveys than would an assigned value, an assumption which we think untenable except for those cases in which the respondent consistently fails to respond to an item. (Indeed, if there is any substantial number of chronic item non-respondents, an analyst should become suspicious about systematic bias in the data and examine their records quite closely, rather than simply deleting them from the data base.) Where non-response to a given item is not chronic, it seems likely that a properly assigned value will have more research utility, because it preserves the base for sequential data, than a retained non-response. Some methods of determining what values are to be assigned provide rather refined estimates, so that the degree of error in repeated-measures correlations can be rather small.

That portion of the justification given for deleting "small numbers" of item non-response from percentage bases is acceptable. If the proportion of item non-respondents is quite small, it will matter little how far their true state of affairs departs from that of the respondents, since the summary statistics (with some exceptions) can be little affected by small proportions.

But Mayeske (1972) has demonstrated that it is false to assume the equivalence of respondents and non-respondents for some kinds of data. Consider variables for which the possible empirical range is very great

and the item non-respondents are drawn systematically from the tails of the distribution. In such a case, means and variances might be markedly altered by omission of the item non-respondents.

Such effects would be trivial in most cases, although one might be concerned if, say, all item non-respondents, constituting (e.g.) 4 per cent of the cases, happened to make up an extreme class, such as the class of all families with annual incomes above $50,000 or of persons with postgraduate degrees. Deleting item non-respondents in such a case would yield a percentage distribution in which those classes would be empty. It may be that such an unusual form of systematic bias never occurs, but the fact that it could occur should make a researcher wary of adopting a policy on grounds like those put forward by the Ohio State group.

The TALENT group's approach, even though imperfect because item non-respondents are mingled with other unspecific responses, is preferable: include non-response in the percentage base, and report it as a separate category. This at least gives the research consumer some idea of the possible kinds or degree of error which may exist in the reported distributions, and gives him the opportunity to recalculate a distribution using only completed cases.

Non-assignment: a summation. The three examples just critiqued suggest that justifications for abstaining from assigning values to missing cases tend to be absent or weak. When a researcher has only

the crudest of methods for determining values, or when item non-response is quite low, he might be better off not to assign. But even in the case of analyses based on comparison of individual scores over time, the fact that correlations may be affected by assigned values seems less a problem than the alternative fact, that without assignments there can be no use of cases where either of the correlated values are missing. With or without assignment, estimates of population relationships may be biased, but the researcher who assigns values according to some well-grounded scheme stands a better chance of avoiding serious bias than his colleague who allows the non-respondents to determine what portion of the population goes unrepresented. It would seem to be a rather exceptional case, then, in which no assignment of missing values should be made.

The strongest justification for nonassignment would seem to exist when comparisons are to be made among irreducibly small subsets of a large data base, i.e., those subsets for which no internal differentiation can be made and whose means or medians would, therefore, serve as the assigned values for all missing cases within the subset. In such an instance, little would be gained by adding to the number of cases and one would lose variability which could be essential to the analysis. Even in such cases there might be practical reasons, such as maintaining equal cell frequencies for ANOVA, why the researcher would prefer to lose some variability rather than revise his analysis plan or face major difficulties in the processing or interpretation of data.

Perhaps the best reasons for preferring assignment lie in the realm of the researcher's control over the representativeness of his data. Abdication of control over sample representativeness, which is implicit when no assignments are made, seems hardly desirable as a research stratagem when there are workable alternatives. In the following section we consider four cases where some form of assignment was used.

1970 Census.

Source: U. S. Bureau of the Census; Census of Population: 1970; Vol. I, Characteristics of the Population. Part 1, United States Summary - Section 2. Washington: Government Printing Office, 1973 (Appendix C, pp. App. 67-69)

The decennial census is intended to provide cross-sectional, descriptive data. In this respect, it differs from the others under consideration and from the proposed uses of the NLS-HS survey. For present purposes, however, the Census must be considered since its approach to dealing with item non-response is a standard that may be presumed grounded on well-conceived and well-executed policy. This presumption, of course, does not imply that the Census technique is necessarily applicable to other data bases. As it happens, however, the Census approach corresponds in important respects with the approach taken by the University of Michigan Institute for Social Research, which we consider next.

Census allocates missing data in two ways, only the second of which concerns us here:* (1) by substitution of a complete record of one household for that of another dwelling unit which is determined to be occupied but from which no response was forthcoming, and (2) by using a "hot deck" (random match) as the source of a value for missing data in item non-response.

The Census version of this random matching procedure is possible only with special computer programs that cause selected complete records to be temporarily stored in memory.** Each such stored record is replaced by the next-appearing record that matches it on selected characteristics, so that the particular record in storage at any point in time is a function of the order in which data are processed. If the order is random, the record in storage at any moment is to all intents randomly chosen from the population of all records having a prescribed combination of characteristics.

The combination of matching characteristics depends upon what item of information is involved as the dependent variable. In the case of income (for example) characteristics such as sex, race, age, geographic location, education, occupation, and the like might be considered relevant variables, and records representing all possible combinations

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* The first technique applies to general, rather than item, non-response and is therefore outside the scope of this commentary.

** However, the basic "hot deck" approach, involving random selection of a value, might conceivably be used by almost any researcher.

of coded values would be held temporarily in memory. These "reference records," as we shall call them, are records containing values for the variable of interest (e.g., income). At any given moment, then, the computer will have on file a record containing all information needed (i.e., in use) to assign a value for income. *

When a record missing a dependent-variable value appears, it is matched (as closely as possible) with one of the "reference records," and the value for the "reference record" is assigned to the missing data case. In the ongoing example, a record lacking income data would be matched with a reference record on the basis of the predictive characteristics, and the income contained in the reference record would be assigned as the income of the item non-respondent. The item non-respondent's revised record then replaces the initial "reference record," and itself is stored in memory until the appearance of another complete record with the appropriate combination of "match" variable values.

The end result of the "hot deck" procedure, applied to a very large data pool, is that the mean and distribution for the assigned cases within any characteristics-defined subset will approximate that for the known-data subset to which it has been matched. This follows from the fact that the assigned values are in effect randomly selected from within

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* Needless to say, for dependent variables other than income, there would likewise be sets of "reference records" with complete data on the dependent and the "match" variables.

the range of the known cases for a subset of the population. *

This approach clearly differs from the more common method of assigning a known-subset mean to all item non-respondents, since the latter eliminates all variability among the assigned cases although, of course, it too yields the known-subset mean as the mean of all assigned cases.

It is to be emphasized that the Census approach is designed for purposes of cross-sectional description and to offset consistent biases in item non-response. Thus, if the poorly educated portion of the population tends to omit (in our example) income information, the "hot deck" procedure compensates for this tendency. It yields improved estimates of the proportions of the population with lower incomes, because it takes advantage of known correlations between income and "match" variables such as sex, race, education and the like.

It should be clear that the Census approach would not be suitable for panel studies, because of the random nature of values assigned to individual missing-data cases. Were it used with panels, repeated-measures correlations could be affected quite markedly, for the degree of error in the assignment of any one case is likely to be large. (Even though the aggregate error involved is no more likely to be large than is

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* The "reference records" may be regarded as having been weighted by a factor of 2, since their criterion values are entered twice into the data pool.

the case when subset means are assigned.)

It is entirely possible that, in our example, an item non-respondent whose actual income is in the top five per cent of the subset distribution will be assigned a value from the bottom five per cent, introducing a large error for that record. Though the incidence of such extreme errors would not be large, it may be supposed that 68 per cent of the assigned cases could have values departing from their true value by one to two, and the other 32 per cent could have values erring by three to four, standard deviations. The consequences of such potentially great errors for dynamic analyses are apparent. As stated in Parnes, et al. (1971), "... such allocated data could not be expected to be consistent with data from subsequent surveys." It is this consideration which rules out direct application of the "hot deck" method for panel studies.

Panel Study of Income Dynamics.

Sources: (1) Finlayson, S. (ed.) A Panel Study of Income Dynamics: Study Design, Procedures, Available Data -- 1968-1972 Interviewing Years (Waves I - IV). Vol I. Ann Arbor: University of Michigan, Institute for Social Research, 1972. (pp. 273-321)

(2) Morgan, J. N., K. Dickinson, J. Dickinson, J. Benus, and G. Duncan; Five Thousand American Families--Patterns of Economic Progress. Vol. 1: An Analysis of the First Five Years of the Panel Study of Income

Dynamics Ann Arbor: University of Michigan, Institute for Social Research, 1974.

Of all the studies examined, the most extensive discussion of item non-response, and we think the most sophisticated approach, is given in the above study. The ISR employed a staged approach to data assignment, running from judgments based on other information supplied by the item non-respondent* to assignments from tables calculated by a statistical algorithm called AID (automatic interaction detector). Assignments were assessed and coded in "minor" and "major" categories, according to the degree of probable error: minor assignments were those for which probable error was under $300 or less than 10 per cent of the value of the variable, and major assignments were those for which error was at least $300 or 10 per cent of the value of the variable.

The use of the AID procedure is the most interesting aspect of the assignment procedure. As described in Morgan et al. (1974:359-62),** the AID procedure resembles the Census approach in that it uses a set of predictor variables which describe some subset of the sample of known responses, whose characteristics can be employed to determine a criterion value for assignment to missing cases.

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* Editing assignments were, like those described by Parnes et al. (1971), made on the basis of examining other responses in the interview protocol or by reference to information supplied in earlier interviews.

** A more complete description is provided in Sonquist et al. (1973) and a description of the related THAID appears in Morgan and Messenger (1973).

The AID procedure differs importantly from the Census "hot deck" procedure in three respects. First, whereas the Census employs a fixed set of predictor variables for any one criterion, the AID program may yield differing sets for every value of the first (most important) predictor, and likewise for each subsequent predictor. Thus, the AID, by taking advantage of interaction among the predictors, is capable of producing a very refined subset structure for use in "matching" item non-respondents. Second, whereas the selection of predictors employed by the Census is based upon externally known correlations, the AID procedure searches and substructs the given data set to locate those subsets of characteristics which predict best for the body of data under immediate consideration. Third, whereas the Census procedure assigns randomly matched individual values of the criterion variable, the AID procedure yields subset means which may then be assigned to missing data cases. An example of the output of the AID procedure is shown in Figure 3, as it appears in Morgan et al. (1974:48).

The meaning of the variables in the example need not concern us here. The focus of interest is the variation in predictors at the third and fourth levels of the chart (race vs. ability test scores at level 3, four different variables at level 4) and the marked variation in criterion means for every contrasted value* of a given variable.

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* Continuous values are bracketed into a small number (5-10) of categories. The AID program examines all possible dichotomous splits for each independent variable.

Average Annual Change in Income/Needs as Per Cent of Five Year Average
(for all families with a different head in 1972 from 1968)

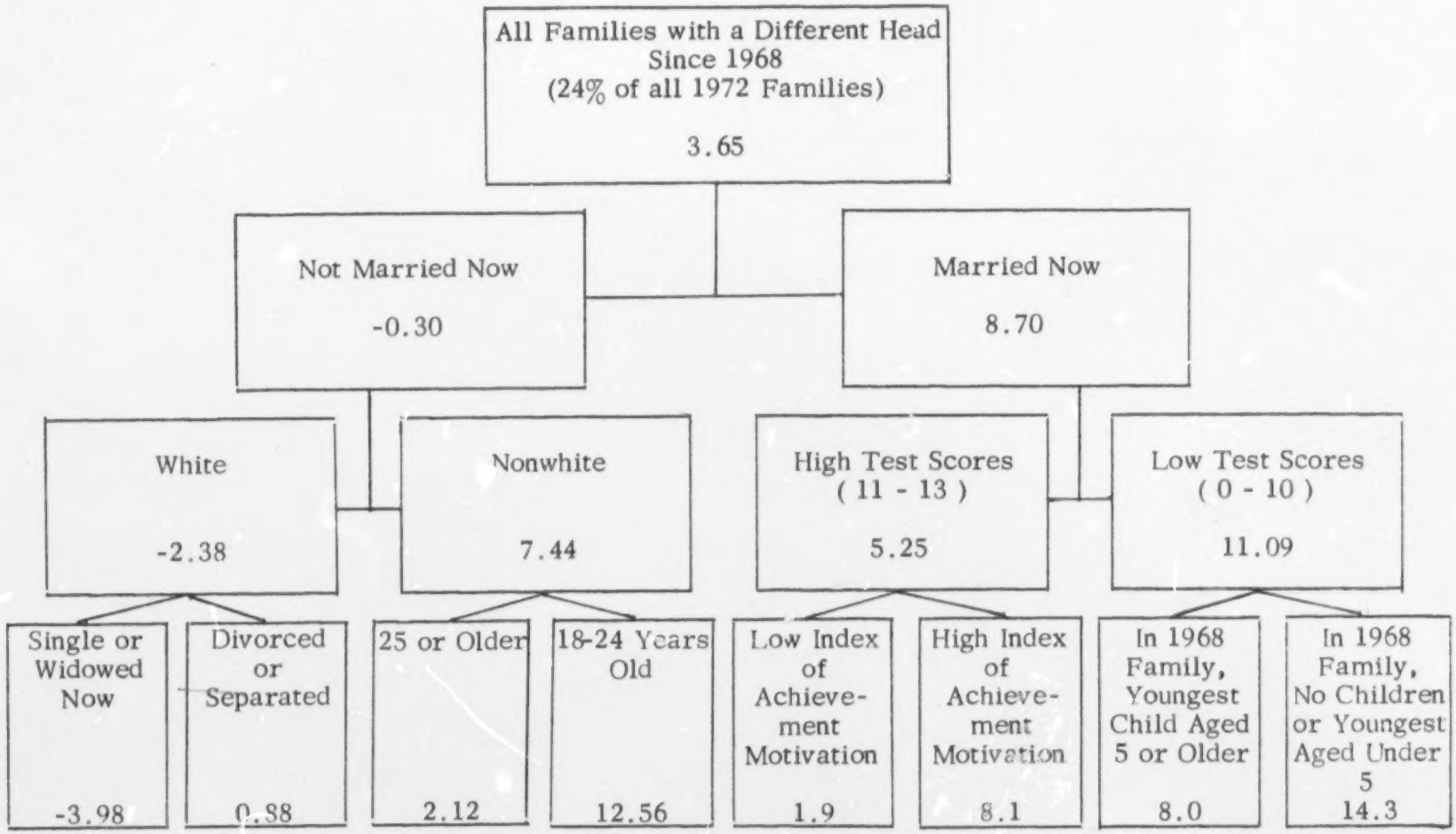


Fig. 3.--Example of AID output.
Source: Morgan et al. (1974:48)

It is evident that assignment of mean scores for missing data would be much more precise with breakouts produced by the AID program than with more ordinary approaches which use a fixed set of descriptors. In Figure 3, the breakouts shown are those which best differentiate the sample, to account for the greatest portion of variability in the criterion score. The procedure informs the analyst what variables (from a set of selected candidates) to "match" on and what criterion mean to assign for item non-respondents with given combinations of characteristics.

The ISR assignment method is a variant on the standard procedure of assigning subset means. Its particular suitability, as a possible treatment of the NLS-HS data base lies in the use of the AID program to help specify what means to assign.

Like most other routinized (especially, machine-performed) procedures, the ISR approach has certain intrinsic problems. Reliance on the AID to produce subset means would require great care to assure that assignments are not unintentionally compounded. (An analyst might choose to allow compounding, however.) The program does not automatically discriminate between assigned and actual item values. It is conceivable that routine use of all cases for which a value is available might result in AID analyses based on a large proportion of assigned values. In that case, its power would be vitiated at best or, at worst, its outputs might be unreliable. Some protection against compounding assignments is needed, which (we presume) is one reason for special

"flagged" coding of assigned values in the ISR approach. Clearly, the problem of compounding by routine machine processing can be forestalled by appropriate coding and programming. But NCES would have to warn prospective users of the approach that some such protection is needed, and might well suggest appropriate safeguards.

Educational Opportunity Survey - Office of Education.

Source: Mayeske, G. W., C. E. Wisler, A. E. Beaton, Jr., F. D. Weinfeld, W. M. Cohen, T. Okada, J. M. Proshek, and K. A. Tabler; A Study of Our Nation's Schools Washington: DHEW - Office of Education, 1972.

The Mayeske group's analysis of data from the survey which generated the Coleman Report (see above) employed an unusual method of assigning values to item non-respondents. For reasons other than data assignment, Mayeske wished to create variables scaled in a common metric from a diverse set of available variables. To do so, he selected an intrinsically interesting "outcome" or criterion variable, and performed criterion scaling.

In brief, Mayeske computed the criterion mean for each category of nominal variables, and/or for each value (or, bracketed interval) of continuous variables, within a set of variables chosen for use in factor analyses and regressions. These variables were then scaled in terms of the associated criterion value. The procedure not only put all the "independent" variables into a common metric, but allowed nominal variables

to be represented in interval form. For example, the distinction between males and females might be represented as means of 4.5 and 3.0 in terms of a criterion score.

As a byproduct of the scaling procedure, Mayeske was able to determine the criterion mean for item non-respondents on the independent variables. Like any category of item respondents, item non-response could be represented by some mean criterion score. Mayeske found that, for many independent variables, the item non-respondents scaled quite differently from any of the several categories of respondents. (See Mayeske et al., pp. 10-11.)

Like the Michigan ISR approach, Mayeske's group is, of course, using a variant of the traditional method of assigning a category mean to item non-respondents. The unusual aspect of the approach is that it does not assign the mean for some "matched" group of respondents, but provides a value unique to item non-respondents.

While it has much appeal, this approach must be criticized on grounds of theoretical value. Treating item non-respondents as a separate category masks as much as it reveals, and information about differences in the criterion scores of item non-respondents and respondents has virtually no theoretical value. If the purpose of education research is to develop predictive models about relationships between certain individual or group characteristics and (say) academic success, there is no way to incorporate "non-response to an extra curricular activities

item" as a variable in the model.

For correlational analyses (in which factor analysis may be included), the Mayeske approach is valuable insofar as it maintains the size and representativeness of a sample, thus permitting greater confidence in estimates of associations between dependent and independent variables within the population. But when specification of complex (multivariate) relationships is the objective of the analysis, criterion-scaled scores for item non-respondents would seem merely to muddy the analytical waters.

The Mayeske group suggest (p. 11-12) that knowledge of a criterion score for an individual may permit inferences about other characteristics which have been criterion scaled. We agree, but would raise a question about the accuracy of such inferences if the variability within the item non-response category is large (i.e., if the fact of non-response is poorly correlated with other personal characteristics). Mayeske gives the example of estimating father's occupation level from a criterion score by comparing the "don't know" mean with that of means for known categories of father's occupation. Examination of his table 3.3.1.1 (p. 10) shows that for twelfth graders the "don't know" criterion mean most closely approximates that for students whose fathers are farm workers (as does the mean for item non-response). In this instance, because there is a roughly linear relationship between father's occupational level and the criterion, Mayeske et al. suggest that a relatively

low criterion score might be used to assign "one of the lower ranks" to (missing) father's occupation.

Mayeske's data show that no occupational category but "farm worker" even roughly approximates the "no response" criterion score. Given this, one would be forced to assign the rank appropriate to "farm worker." But, because the occupational status of "farm worker" is vague, and because the link between Mayeske's criterion (achievement composite score) and "father's occupation" is imperfect, following Mayeske's suggestion would produce doubtful assignments.

For Mayeske's data, occupations are ranked by criterion score (at grade 12 level) as shown in Figure 4. The location of "farm worker" in this ranking does not correspond well with its place in the NORC Scale of Occupational Prestige. In that scale, "farmhand" ranks somewhat above several semi-skilled jobs (coal miner, taxi driver, restaurant waiter, bartender), rather than far below the semi-skilled level as in Mayeske's ranking. Since there is much evidence for the validity of the NORC prestige score as a correlate of income and education, we tend to trust the rankings it produces more than those given by Mayeske's criterion scaling procedure.

It seems very likely that an ad hoc criterion such as that used by Mayeske would produce many misclassifications. To avoid them, the analyst would have to use only a few very broad categories and/or would be forced to make extensive checks on the reasonableness of

| Father's Occupation | Grade 12 Criterion Average | Rank |
|---|---|---|
| Professional | 56.0 | 1 |
| Salesman | 53.6 | 2 |
| Manager | 52.8 | 3 |
| Official | 52.7 | 4 |
| Technical | 52.4 | 5 |
| Farm or Ranch Owner or Manager | 50.7 | 6 |
| Skilled Worker or Foreman | 50.6 | 7 |
| Semi-Skilled | 49.5 | 8 |
| Workman or Laborer | 47.2 | 9 |
| Farm Worker | 42.5 | 10 |
| Non-response | 42.3 | 11 |
| Don't Know | 41.8 | 12 |
| Average | 50.0 | |

Fig. 4.--Occupational ranking based on child's scholastic achievement scores. Source: Mayeske, et al. (1972:10; Table 3.3.1.1).

assigned values. The need to supplement criterion scores with evidence external to the study suggests that the method cannot suffice as the principal basis for assigning values.

Differences among criterion means for nominal categories, as in Figure 4, may be just too small or too ill-defined to permit reliable assignments from the criterion to other variables. Mayeske et al. themselves seem ambivalent on the matter, since they point out (p.10) that curvilinear relationships or other departures from well-defined linear relationships may make it impossible to use criterion scores for assigning values on other variables. Nonetheless, they subsequently (p. 11) summarize the advantages of the criterion scaling approach with initial emphasis on its potential utility for such assignments.

The basic problem with employing criterion means for purposes of assigning missing-data values, we think, lies in the improbability of a high correlation between the criterion and any one other characteristic variable. Because of this fundamental weakness in social science data, some form of multivariate approach (like the Census or ISR methods) probably will be more adequate as an approach to the problem of item non-response. In enumerating the advantages of criterion scaling, Mayeske et al. should have emphasized the fact that criterion scaling maximizes the linear relationship of the variable with the criterion. We would, then, tend to discount their implied claims for its usefulness in assigning missing data.

Project SCOPE.

Source: Tillery, D. and T. Kildegard Educational Goals, Attitudes, and Behaviors: A Comparative Study of High School Seniors. Cambridge, Mass.: Ballinger Publishing, 1973 (Note: Tillery is the originator of and senior analyst for Project SCOPE.)

These analysts employed the most simple and traditional method of assigning missing values:

> No variable was used for which the nonresponse rate was more than about 14 percent, and in most cases the nonresponse was less than 10 percent. Mean values for each variable were used for subjects who did not respond. (Tillery and Kildegard, 1973:20)

According to this brief statement, the analysts did not even assign subcategory means. Although they seem to say they rejected items with more than 14 per cent non-response, it is more likely they mean "The maximum item nonresponse rate was about 14 per cent, . . ." Since their approach is poorly explained, but appears quite simplistic, we think further discussion is unnecessary.

Assignments: summation. The chief--here, only--justification for not assigning values to item non-respondents is stated in Parnes et al. (1970): " . . . allocated data could not be expected to be consistent with data from subsequent surveys." The procedure used by the Michigan ISR group takes account of the need for caution on this ground by applying special codes indicating assignment and the degree of uncertainty attached

to each assigned value. *

We have argued that a carefully assigned value is probably more useful than no value at all, even in longitudinal analyses. Of those considered, the procedure employed by the Michigan ISR appears to yield the most refined estimates of values, and probably the most reliable. Its acceptability as a variant of the standard approach--assigning subcategory means--should be enhanced by the fact that it requires few a priori judgments about what variables, or what values of these variables, should be employed, as the basis for data assignment.

In theory, all variables other than the one to be assigned could be considered for Michigan's AID procedure if the data base were large enough to support the statistical manipulations required. The analyst, of course, will probably exercise judgment in the deletion of variables from the list of candidates on the basis of expert knowledge or theoretical grounds. For technical reasons as well, some selectivity would be required. Few data bases are likely to be large enough to make possible the simultaneous consideration of all of a large number of available variables, and the AID computer program limits the number of variables allowable for any one run.

In sum, although it has certain limitations which it shares

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* These codes, of course, do not enter into computations, but may be used to warn data tape users that the value is in doubt. This is probably the best that a data archive can do for its users.

with any assignment method, and some which are peculiar, the ISR's use of the AID procedure to determine what means should be assigned seems an approach which we can recommend for consideration in the adjustment of the NLS-HS data base.

Need for empirical studies of item non-response bias.

Is high non-response a real problem? We stated at the outset that each researcher must decide for himself whether or not to assign data. We favor doing so, especially when non-response is fairly high for an item and when there is reason to believe that some systematic bias is involved. From a social psychological standpoint, we can probably assume that non-response implies bias. The fact of non-response distinguishes all non-respondents from all respondents. Whether or not bias is systematic, i.e., whether the motive for non-response is similar across individuals or whether they share relevant characteristics, is a matter which must be determined empirically in each case, as was done by Mayeske and by Blau and Duncan (1967:471-76).

The data provided by Mayeske provide strong evidence that item non-response can introduce very serious biases. But comparisons of certain Census distributions, with and without adjustments for item non-response, indicate that assignment sometimes yields rather minor changes even when item non-response is as high as 21 per cent. For non-response in the range 4.5 to 11.7 per cent, Blau and Duncan (1967:474) likewise found negligible effects.

This contradictory evidence raises doubt that high rates of item non-response are necessarily a problem. A cursory examination of the adjusted and unadjusted Census data for four variables, as indicated in Tables 4 to 7 (appended) shows that the average change in cell proportion resulting from missing-data assignments was only about 0.17 per cent, regardless of the proportion of missing cases (from about 4 per cent to about 21 per cent in the four tables). The biases in the unadjusted distributions were systematic, but rather minor. When the change for each cell is related to the original cell proportion, the modification resulting from adjustment is about 2 per cent* of the unadjusted cell proportion.

As might be expected, the size of the effects of assignment is proportional to the number of categories in a distribution and greater for less populous cells. The effects do not seem to depend, however, on the proportion of missing-data cases, as comparisons of Tables 4 and 5, and of 6 and 7, show. It might be supposed that systematic biases would be more severe as the proportion of item non-respondents increases, but this is not the case for the Census sample data.** Whether or not this is owing to the huge size of the data base, to the allocation procedure employed, or to some combination of these is uncertain.

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* Simple mean, over all cells in column 6 of Tables 4 to 7 collectively.

** The data of Tables 4-7 are based on approximately 20 per cent of 1970 households. The figures given there are weighted estimates of population distributions.

Blau and Duncan (1967) were concerned about the impact of item non-response upon correlations underlying their investigation of occupations. They report an effort to assess the effects of systematic bias in the characteristics of those not reporting "father's occupation" and "respondent's first occupation." For the age group they examined, non-response rates for these variables were 11.7 per cent and 4.4 per cent respectively. Their results show both means and variability were reduced for a mixture of persons who failed to report at least one variable.* For "father's occupation," the "nonrespondent" mean was 90 per cent of the "complete data" mean. For "respondent's first occupation," the corresponding figure was 84 per cent. Curiously, the influence of non-response was less for the variable with greater non-response.

By an elaborate procedure, Blau and Duncan estimated a value for the likely "true" population correlation, under varying assumptions about the unknown correlation between "father's occupation" and "respondent's first occupation." By their estimate, the influence of item non-response was minor, probably about the same size as the sampling error of "r" for their large (N = 33,000) sample. Thus, in this instance, they concluded that bias attributable to item non-response was not a matter of concern.

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* Computed "nonrespondent" means and deviations were based on imputed values for those not reporting the variable in question and obtained values for those not reporting the other variable (of the pair considered).

They express concern, however, for other portions of the data with item non-response of about 20 per cent, remarking that correlation coefficients are "especially vulnerable" to misestimation for these.

Given contradictory evidence, we recommend that NCES undertake some special empirical studies of the degree of bias introduced by item non-response in the NLS-HS data.

Since it is too late to conduct personal interviews with samples of first follow-up item non-respondents (and doubtless too costly to select samples on an item-by-item basis in any event), a reasonable approach to such empirical work might be to construct a subsample of known data cases, representing the survey respondents, then to delete cases so as to re-create the experienced item non-response. Comparison of the data from the whole subsample with the data after deletion of cases would provide some estimate of the effects of item non-response.* Or, NCES might wish to repeat Blau and Duncan's analysis, using especially troublesome NLS-HS data. This preliminary step might at the same time provide a data base on which alternative methods of data assignment could be tried, to assess consequences of their use.

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* Provided, of course, that not only the rate of item non-response but also the relevant characteristics of non-respondents (as best known) were re-created in the experiment suggested. Cases would be sampled within characteristics-defined subcategories to create the hypothetical "item non-respondents."

A possible method of reducing dependence on "expert judgment" as a basis for assignments. Procedures such as the A.I.D. program help reduce the bounds within which judgment must be exercised, but even this rather sophisticated approach does not replace judgment. Although we have given some thought to the matter, we have not conceived of any method which would allow complete elimination of judgment by empirical evidence.

A definitive study would require knowledge of the reasons for item non-response and an assessment of the relationship of each of the several reasons with the "true" values of the missing data. Given the data presently available, such an analysis cannot be performed. Were there sufficient concern to warrant the expense of special follow-up studies, reasons for non-response might be obtained from item non-respondents in future waves of the NLS-HS survey.

For certain items, interest in improving accuracy of the data base might justify such an effort. "Reasons why" information, however, is notoriously subject to various forms of distortion owing to factors such as socially acceptable response, rationalization, or creation of artificial justifications.* Given such problems, plus recall error, the value of follow-ups aimed at discovering motives for item non-response seems

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* That is, the respondent makes up something to satisfy the inquiry, even though there was not--or he cannot specify--any particular reason for non-response at the time it occurred.

doubtful.

It might, however, be worthwhile to make reasonable estimates of the motives underlying failure to respond and, from such estimates, to narrow the scope of required judgments. The approach sketched below would be time-consuming and costly; therefore, consideration of its use should be limited to items which are of critical importance and have unacceptably high rates of item non-response.

We assume, as the basis for this approach, that different reasons for item non-response will be associated with varying "true" values for the variable in question. That is, we suppose that those non-respondents who intentionally conceal data will tend to differ from those whose non-response is the result of error in following skip patterns, etc. We suggest that non-respondents can be described in terms of certain patterns of response to the total questionnaire, or to several follow-ups, as well as in terms of their personal or contextual characteristics.

Mayeske et al. have shown that the mean criterion scores for non-respondents (undifferentiated as to motive) tend to differ from those for specified categories of respondents. Our suggestion takes this evidence as the basis for the assumption that differently motivated non-response will likewise exhibit differences in item values. The problem, of course, is that for the item in question values are not available for non-respondents. Thus, some way of estimating appropriate values for each type of non-response must be found. Our suggestion is a multi-stage

procedure which might yield fairly refined estimates. It is a variant on the standard method of assigning means or other measures of central tendency.

The THAID algorithm used by the University of Michigan Institute for Social Research forms the basis of the method. This procedure* locates those variables, in a set of candidates, which maximize the difference in distributions of cases over a set of categories for a criterion variable. By iterations, the program yields information about which candidate predictors are the most powerful (in terms of differentiating the distributions) and what values of each predictor are associated with varying distributions. The program seems uniquely suited to analysis of item non-response in terms of estimated motive, as discussed below. It enters into the overall procedure in the final stages.

The steps necessary to the procedure are:

1. Classification. If we assume that item non-response can stem from any of the several sources like those listed below, the first step would be classification of each item non-respondent into one of several categories, on the basis of an edit of the questionnaire:

   a. Administrative error - questionnaires with missing pages, illegibly printed pages or items, or the like, which can account for item non-response.

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* A brief description of the program is given in Morgan, et al. (1974) and a detailed account appears in Morgan and Messenger (1973).

b. Respondent error - indicated by evidence of respondent difficulty in following the questionnaire, such as frequent routing errors, failures to follow item instructions, many inconsistent responses, and the like.

c. Respondent lack of information or indecision - indicated by patterns of response which suggest that, though cooperative, the respondent is unable to provide specific information. Such patterns might include frequent use of "don't know," "undecided," multiple responses, and the like.

d. Respondent deviance - indicated by patterns of response which suggest that response options provided are inadequate to the peculiar situation of the respondent, such as frequent use of uncodable or "other" responses.

e. Limited time/patience - patterns which indicate that the respondent simply quit responding, after having done so at the outset: all item non-response concentrated in "blocked" portions of the questionnaire, with complete and consistent responses in other portions.

f. Intent to mislead - patterns of response which suggest that the respondent intended to mislead or simply confound the analyst, such as frequent "out of range" responses, highly unlikely single responses or combinations, including face sheet items (e.g., Puerto Rican ethnicity and Shinto religion), and the like.

g. Intent to conceal - patterns of omission which "flag" certain items in a block of related items as intentionally omitted to conceal; for example, failure to supply income in the midst of completed answers on other employment or standard-of-living items, especially if no other motives are suggested.

h. Mixed motives - presence of indications that item omission is probably part of more than one pattern of motivation, such as both error and lack of information or indecision.

i. Indeterminate - a residual category covering cases for which no clear patterns are found.

Interviewer notes and comments might be used to supplement study of response patterns in the categorization of item non-response motives.

2. Selection of candidate predictor variables. Indiscriminate inclusion of all available variables among the THAID candidates would be inefficient. Since the final step involves relating subcategories to item values, only those variables which are highly correlated with the item under consideration should be included among the candidates. Thus, the second step of the procedure calls for an examination of correlation matrices,* derived from the item respondents, to determine which variables

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* We use the term somewhat loosely. The matrix would have to contain a mix of various measures of association, not necessarily the Pearson 'r' often suggested by "correlation."

have a high zero order correlation with the criterion (motive). Candidate variables should have this property and their intercorrelations should be relatively low. From a large number of variables, perhaps as many as 15 might be selected for final inclusion. It is to be emphasized that the selection procedure is wholly empirical--there need be no interpretable "reason" for high correlation between the criterion item and a candidate variable, since the objective is confined to prediction.

3. The candidate variables are entered as predictors in the THAID program, with non-response motive as the categorical dependent variable. The program selects combinations which best discriminate distributions, thus yielding "best estimates" of characteristics associated with membership in a motive category.

4. For each category of motivation, the combinations of characteristics yielded by THAID can be utilized to identify a subset of item respondents, for which a summary statistic--mean, median, mode--can be computed.

The chief advantage we see in such an approach is that it seeks to take account of motives for non-response as a variable plausibly associated with criterion item values. It departs from traditional ways of assigning means only by considering information besides the customary background characteristics of the item non-respondent as a basis for matching him to some subset of respondents. Where such motives as intent to mislead or to conceal underlie item non-response, there is good

a priori reason to suppose that some peculiarity in the respondent's situation, directly affecting his true item value, has induced the omission of the item. Likewise, respondent error and lack of information may reflect personality or ability factors which, in turn, may bear upon the respondent's experience and his standing relative to otherwise similar respondents. We would, of course, like to be able to spot deviant cases, those respondents whose circumstances depart so far from the norm that precoded response options are inadequate. For such reasons, there seems justification--for crucial and high non-response items--to undertake some effort like the one suggested.

Closing comment. The "state of the art" of adjusting data for item non-response appears primitive, despite the existence of some rather sophisticated techniques. What we have found wanting are not procedures for manipulating data, but rather statements of the logical underpinnings and accompanying empirical evidence of the consequences of data assignment. At present, each researcher seems on his own except for traditional--but not well examined--treatments.

It is especially unfortunate that what efforts have been made appear to focus chiefly on adjusting distributions to compensate for errors in static population description induced by missing data. The potentially more important matter of adjusting individual records, for longitudinal analysis of processes, seems almost unexamined.

We think NCES or its contractors would make a significant

contribution to both the value of the NLS-HS data and the state of the art of longitudinal analysis by such methodological studies as those we have sketched. Hence, our strong recommendation that such efforts be undertaken.

## POSTSCRIPT

This paper in draft form has stimulated discussion of the problem of item non-response and data quality among present and prospective users and the governmental and private organizations responsible for the NLS surveys.

Those discussions have generated some points of agreement as well as some controversy. All participants appear to accept the critique of the questionnaire as too detailed and too complex for a mail-out survey. Yet there seems little possibility that any major improvement can be made for the third follow-up and, we are told, it is likely that the questionnaire will be even more difficult in that wave, because Federal agencies with an interest in the cohort sampled have succeeded in adding items to the survey. None have been willing to delete any of the details sought in the first follow-up. Whether this survey can bear the burden of gathering so much disparate information remains to be seen. We have doubts, even though the contractor has planned for telephone call-backs, to obtain critical information, for about half the sample respondents.

Some of the difficulties cited in this paper have been corrected

retroactively, and some modifications in the questionnaire graphics have been made. Conditional item responses now are coded, where required, to include a flag for inconsistency with the "parent" routing item. In the second and later questionnaires, SKIP instructions have been reworded to "GO TO" and printed in red.

Despite these useful modifications, many coding and format problems remain. The survey contractor is considering our suggestions on coding and formatting, but will be unable to test any of the latter for possible use in the third follow-up because necessary instrument approvals and logistical preparations cannot be changed so shortly before field pretests are to be conducted.

As might be expected, considerable controversy has been raised about suggestions concerning preparation of an analysis-oriented data file, especially on the possibility of including assigned values for missing data. The National Center for Education Statistics, the responsible Federal agency, opposes the assignment suggestion on grounds like those given by Parnes, et al. (see text, p. 88) and on grounds of cost. Others join NCES in arguing that the "state of the art" provides no generally accepted method for estimating the values to be assigned (a point we stress in the text). One participant opposes the suggestion because researchers with differing problems may wish to use methods other than those which might be adopted for creation of the analysis file.

Some comment on these objections is warranted. We stress

repeatedly that the documentary file should be retained to accommodate researchers who wish to devise their own methods, and it is evident that an assignment "flag" code would permit such researchers to ignore assigned values in a file. Our objections to the Parnes position are given in the text, but we add that Parnes' position was taken with reference to a data base which differs in important respects from the one under discussion. The Parnes base has item non-response rates much lower than those cited here (rarely exceeding 10 per cent) and its data were obtained chiefly by personal interviews conducted by Census-trained personnel. Under such circumstances, the policy on missing data might well differ considerably from what is appropriate for the surveys of the Class of 1972.

The most cogent objections to data assignment are those based on "state of the art" and cost. The concluding portion of our paper discusses critically the assignment of missing data, and recommends a program of methodological studies intended to investigate whether <u>any</u> method of data assignment will markedly affect population estimates and, if so, which method seems most appropriate for this data base.

Such a program would be costly, and its results might not yield assigned values acceptable to all users. Nonetheless, we still assert that some effort to "fill in" missing data is highly desirable for longitudinal analysis, so long as the estimation/assignment procedure chosen provides well-grounded and clearly flagged values.

We have pointed out in discussions that a decision to omit assigned values has serious cost implications for users, some of whom will perform their own (possibly duplicative) adjustments of the data. Some analyses may be foregone because otherwise competent analysts lack data processing facilities or skills to modify the data. Some misleading policy "information" may flow from analyses based on that self-selected portion of the sample which responded to a particular item or set of items.

Against the background of a study which reportedly has cost upwards of five million dollars thus far, the expenditure of time and funds to assess methods of data assignment seems well justified. The benefits flowing from these costs would be a data base accessible to a wide variety of potential users, some assurance that information based on the NLS data is grounded on the best estimates that current survey-methodology can provide, and a substantial contribution to the "state of the art" of longitudinal survey analysis.

Clearly, NCES should not offer only a data file bearing assigned values. Neither should it provide assigned values or a manual for making assignments without first pursuing the necessary methodological studies upon which to ground its recommendations. Although we have been audacious enough to recommend one particular method among those we reviewed, we urge the NCES launch its own investigations and draw others into the discussion. We hope that many interested parties will volunteer empirical evidence and/or opinions, so that the debate can be intensified.

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TABLE 2

BASIC ITEM CONTENT, BY RATE OF USABLE RESPONSE AND NUMERICAL SEQUENCE

95 to 100 Per Cent

| Item Number | Paraphrased Content |
|---|---|
| F21 | Any training program after high school? |
| F48A[1] | Was respondent working in October 1973? |
| F58A | Number of weeks worked, October 1972 to October 1973 |
| *BY2 | Type of high school program |
| *BY5 | High school grades |
| *BY8 | Average weekly hours worked during high school |
| *BY83 | Any work-limiting physical handicap? |
| *BY84 | Respondent's race or ethnic group |
| *BY92 | Respondent's religion |
| *BY94A) to *BY94K) | Parental home possessions |
| *BY95 | Base year residence area type and size |

90 but less than 95 Per Cent

| | |
|---|---|
| F1A | Present activity: working |
| F1C | Present activity: taking academic courses at a college |
| F4 | With whom living, October 1973 |
| F5 | Kind of dwelling, October 1973 |
| F6A | October 1973 residence area type and size |
| F6B | Distance, October 1973 residence from base year residence |
| F9 | Was respondent financially dependent in October 1973? |
| F10 | Number others financially dependent on respondent, October 1973 |
| F12 | Schooling aspirations |
| F14 | Schooling expectations |
| F16A | Expected activity, October 1974: working |
| F19 | Expected occupation, at age 30 |

See notes at end of Table 2, p. 132.

TABLE 2 (Cont'd)

BASIC ITEM CONTENT, BY RATE OF USABLE RESPONSE AND NUMERICAL SEQUENCE

| Item Number | Paraphrased Content |
|---|---|
| F25 | Was respondent taking courses at any school, first week of October 1973? |
| F49A | Kind of job held, October 1973 |
| F49G[2] | Currently working in this job? |
| F54A[2] | Was respondent working in October 1972? |
| F58C | Number of employers, period October 1972 to October 1973 |
| F78A[3] | Father's education |
| F78B[3] | Mother's education |
| F80A | Did mother work when respondent was in high school? |
| F81 | Did respondent apply for college admission before October 1973? |
| | 85 but less than 90 Per Cent |
| F1B | Present activity: taking vocational or technical courses |
| F1D | Present activity: on active duty in Armed Forces or in service academy |
| F1E | Present activity: homemaker |
| F1F | Present activity: unemployed |
| F7A | Marital status, first week of October 1973 |
| F11B[4] | Spouse's total 1973 income |
| F13A | Amount willing to borrow for schooling |
| F16B | Expected activity, October 1974: taking vocational or technical courses |
| F16C | Expected activity, October 1974: taking academic courses in college |
| F22AA | Type training program since high school: on-job training |
| F22C | How long did training program last? |
| F22D | Has respondent completed training program? |
| F22E | Has respondent used training on any job? |
| F23[5] | Has respondent attended any kind of school since leaving high school? |

See notes at end of Table 2, p. 132.

TABLE 2 (Cont'd)

BASIC ITEM CONTENT, BY RATE OF USABLE RESPONSE AND NUMERICAL SEQUENCE

| Item Number | Paraphrased Content |
|---|---|
| F29A[6] | Was respondent taking courses at any school during October 1972? |
| F39 | Has respondent attended any other school since high school? |
| F42 | Was respondent working toward any degree, certificate, or license, first week of October 1973? |
| F43 | Since leaving high school and before October 1973, has respondent earned any certificate, license, diploma, or degree? |
| F48C | Was respondent looking for work, September 1973? |
| F54C | Did respondent look for work, October 1972? |
| F55A | Kind of job held, October 1972 |
| F58B | Number of weeks unemployed, period October 1972 to October 1973 |
| F79 | Father's occupation |
| F80B | Did mother work when respondent was in grade school? |
| F80C | Did mother work before respondent was in grade school? |
| BY90B[3] | Mother's education |
| | 80 but less than 85 Per Cent |
| F2 | Did respondent complete high school? |
| F3A | Month left last high school |
| F3B | Year left last high school |
| F13B | Did anyone discuss borrowing for schooling? |
| F16D | Expected activity, October 1974: active duty in Armed Forces |
| F16E | Expected activity, October 1974: homemaker |
| F22B | Kind of work trained for, in post-high school training program |
| F24P | Reason for not continuing education: earn own money |
| F41B | Did any school attended give credits? |

See notes at end of Table 2, p. 132.

TABLE 2 (Cont'd)

BASIC ITEM CONTENT, BY RATE OF USABLE RESPONSE AND NUMERICAL SEQUENCE

| Item Number | Paraphrased Content |
|---|---|
| F50A | Average weekly hours worked, job held October 1973 |
| F50B | Average weekly earnings, job held October 1973 |
| F56B | Average weekly earnings, job held October 1972 |
| F82B | Admitted to school applied to before October 1973? |
| F82C | Request financial aid, school applied to before October 1973? |
| BY90A[3] | Father's education. |
| | **75 but less than 80 Per Cent** |
| F24A ) to ) F24O) and ) F24Q) | Various reasons for not continuing education after high high school |
| F28A | Field of study (major), October 1973 |
| F30[7] | Did respondent attend the same school in October 1972 and October 1973? |
| F33B | Classified as full-time student, October 1972 |
| F33C | Number of class hours per week, October 1972 |
| F34 | Was field of study the same in October 1972 and October 1973? |
| F56A | Average weekly hours worked, job held October 1972 |
| F83AA | No second-choice school applied to before October 1973 |
| *BY93 | Parents' income in base year |
| | **70 but less than 75 Per Cent** |
| F11A | Respondent's total 1973 income |
| F22AB ) to ) F22AH) | Various training programs in which respondent participated after high school and before October 1973 |
| F26B | Kind of school attended, October 1973 |

See notes at end of Table 2, p. 132.

TABLE 2 (Cont'd)

BASIC ITEM CONTENT, BY RATE OF USABLE RESPONSE AND NUMERICAL SEQUENCE

| Item Number | Paraphrased Content |
|---|---|
| F26C | School attended October 1973 public or private? |
| F27AA | Month first attended school of October 1973 |
| F27AB | Year first attended school of October 1973 |
| F27B | Classified as full-time student, October 1973? |
| F27C | Number of class hours per week, October 1973 |
| F27D | Classified as freshman or sophomore, October 1973? |
| F28B[8] | Field of study October 1973 academic or vocational? |
| F28C | How long to complete program (major) enrolled in as of October 1973? |
| F46AA | Total cost of schooling, first year after high school |
| | **65 but less than 70 Per Cent** |
| F1G | Present activity: other |
| F46AB | Number of months to spend total cost of schooling, first year after high school |
| F47AA | First (listed) source, money for schooling first year after high school |
| | **60 but less than 65 Per Cent** |
| F13C | Was there any change in borrowing plans? |
| F16F | Expected activity, October 1974: other |
| F32C | School attended October 1972 public or private? |
| F37 | Did respondent drop out of school attended in October 1972? |
| F47AB | Amount of schooling money from first-listed source, first year after high school |
| F83B | Was respondent accepted by second-choice school applied to before October 1973? |
| F83C | Request financial aid, second-choice school applied to before October 1973 |

See notes at end of Table 2, p. 132.

TABLE 2 (Cont'd)

BASIC ITEM CONTENT, BY RATE OF USABLE RESPONSE AND NUMERICAL SEQUENCE

Less than 60 Per Cent

| Item Number | Paraphrased Content |
|---|---|
| F11C | Respondent's wage and salary income, 1973 |
| F11D | Spouse's wage and salary income, 1973 |
| F11E | Respondent's scholarship income, 1973 |
| F11F | Spouse's scholarship income, 1973 |
| F11G | Respondent's miscellaneous income, 1973 |
| F11H | Spouse's miscellaneous income, 1973 |
| F29BA ) to ) F29BR ) | Various reasons for not continuing education right after high school (by October 1972) |
| F31A ) to ) F31K ) | Various reasons for changing schools between October 1972 and October 1973 |
| F32B | Kind of school attended, October 1972 |
| F40B | Kind of other school attended, anytime after high school |
| F40DA | Is respondent currently attending this other school? |
| F41CB | Number of semester credits accrued by October 1973 |
| F41CC | Number of other type credits accrued by October 1973 |
| F46BA | Expenditures for tuition and fees, first year after high school |
| F46BB | Expenditure for room and board, first year after high school |
| F46BC | Expenditure for books and supplies, first year after high school |
| F46BD | Expenditure for transportation, first year after high school |
| F46BE | Expenditure for miscellaneous school-related items, first year after high school |
| F47BA | Second source of schooling money, first year after high school |
| F47BB | Amount from second listed source, first year after high school |
| F47CA | Third source |

See notes at end of Table 2, p. 132.

TABLE 2 (Cont'd)

BASIC ITEM CONTENT, BY RATE OF USABLE RESPONSE AND NUMERICAL SEQUENCE

| Item Number | Paraphrased Content |
|---|---|
| F47CB | Amount from third source |
| F47DA | Fourth source |
| F47DB | Amount from fourth source |
| F47EA | Fifth source |
| F47EB | Amount from fifth source |
| F47FA | Sixth source |
| F47FB | Amount from sixth source |
| F47GA | Seventh source |
| F47GB | Amount from seventh source |
| F82DA | Amount of scholarship aid offered, first choice school applied to before October 1973 |
| F82DB | Amount of loan aid offered, first choice school |
| F82DC | Amount of promised job aid offered, first choice school |
| F83DA | Amount of scholarship aid offered, second choice school |
| F83DB | Amount of loan aid offered second choice school |
| F83DC | Amount of promised job aid offered, second choice school |

NOTES: Item content is paraphrased from the wording of the First Follow-Up Questionnaire. Item numbers are those employed for the response distribution published in the User's Manual.

Items prefaced by *BY are background variables for which data was collected from 4,539 individuals via Form B of the First Follow-Up Questionnaire. Data for these cases are included in the published distributions for Base Year Questionnaire variables. Response rates for *BY items thus are based chiefly on data collected in the Base Year administration and are not entirely comparable to those for items collected exclusively in the first follow-up survey.

1 Rate excluding routing-error coded responses is 95.0 per cent; including error-coded responses, rate is 99.5 per cent.

2 Rate excluding routing-error coded responses is 91.4 per cent; including error-coded responses, rate is 99.1 per cent.

TABLE 2 (Cont'd)

BASIC ITEM CONTENT, BY RATE OF USABLE RESPONSE AND NUMERICAL SEQUENCE

[3] Items BY90A (Father's education) and BY90B (Mother's education) are not starred, and are based on data supplied only via the Base Year Questionnaire. They overlap items F78A and F78B (Father's and Mother's education), obtained from all respondents via the First Follow-up Questionnaire. The two items (BY90 and F78) employ different response categories, and response rates are based on different sample sizes (16,683 and 21,350, respectively).

[4] Estimated rate. Published rate = 16.5 per cent, owing to oversized eligible base. Discussed in Sec. 1 of the paper.

[5] Rate excluding routing-error coded responses is 87.5 per cent. If error-coded responses are included, rate is 99.7 per cent.

[6] Rate excluding routing-error coded responses is 85.5 per cent. If error-coded responses are included, rate is 91.6 per cent.

[7] Rate excluding routing-error coded responses is 77.8 per cent; including error-coded responses, rate is 83.4 per cent.

[8] Rate excluding routing-error coded responses is 70.9 per cent; including error-coded responses, rate is 75.4 per cent.

Source: National Longitudinal Study of the High School Class of 1972: Base-Year and First Follow-up Data File User's Manual (Preliminary). Research Triangle Park, N.C.: Research Triangle Institute, April 1975.

TABLE 3

RESPONSE DISTRIBUTIONS, SELECTED FIRST FOLLOW-UP AND BASE-YEAR ITEMS

| Item Number | Number Eligible to Answer (100%) | Usable Responses (%) | Unusable Responses: Routing-Error Codes 20 (%) | Routing-Error Codes 40 (%) | Routing-Error Codes 60 (%) | Unusable Responses: "Garbage" Codes[a] (94 to 97) (%) | Non-Response: PARTIAL RESPONSE (93) (%) | Non-Response: BLANK (98) (%) | Non-Response: LEGITSKIP (99) (number) |
|---|---|---|---|---|---|---|---|---|---|
| F1A | 21,350 | 94.6 | | | | * | 5.2 | 0.1 | 1,048 |
| F1B | 21,350 | 87.3 | | | | * | 12.6 | 0.1 | 1,048 |
| F1C | 21,350 | 90.0 | | | | 0.1 | 9.8 | 0.1 | 1,048 |
| F1D | 21,350 | 86.6 | | | | * | 13.3 | 0.1 | 1,048 |
| F1E | 21,350 | 86.2 | | | | * | 13.7 | 0.1 | 1,048 |
| F1F | 21,350 | 86.4 | | | | * | 13.5 | 0.1 | 1,048 |
| F1G | 21,350 | 65.7 | | | | * | 34.1 | 0.1 | 1,048 |
| F2 | 21,350 | 80.2 | * | 8.1 | | * | | 11.6 | 1,048 |
| F3A | 21,312 | 80.8 | | | | * | | 19.1 | 1,086 |
| F3B | 21,312 | 80.9 | | | | 0.1 | | 19.0 | 1,086 |
| F4 | 21,350 | 93.2 | | | | 0.3 | | 6.5 | 1,048 |
| F5 | 21,350 | 94.0 | | | | 0.1 | | 5.9 | 1,048 |
| F6A | 21,350 | 92.7 | | | | 0.3 | | 6.9 | 1,048 |
| F6B | 21,350 | 94.5 | | | | 0.1 | | 5.4 | 1,048 |
| F7A | 21,350 | 87.1 | 1.2 | 0.5 | | 0.1 | | 11.1 | 1,048 |
| F7B | 6,073 | 55.6 | | | | 0.2 | | 44.2 | 16,325 |
| F7C | 6,073 | 54.9 | | | | 0.2 | | 44.9 | 16,325 |
| F8A | 6,073 | 56.8 | 0.9 | 0.6 | | | | 41.7 | 16,325 |

See notes at end of Table 3, pp. 145-147.

TABLE 3 (Cont'd)

RESPONSE DISTRIBUTIONS, SELECTED FIRST FOLLOW-UP AND BASE-YEAR ITEMS

| Item Number | Number Eligible to Answer (100%) | Usable Responses (%) | Unusable Responses: Routing-Error Codes 20 (%) | 40 (%) | 60 (%) | Unusable Responses: "Garbage" Codes[a] (94 to 97) (%) | Non-Response: PARTIAL RESPONSE (93) (%) | BLANK (98) (%) | LEGITSKIP (99) (number) |
|---|---|---|---|---|---|---|---|---|---|
| F8B | 3,739 | 27.7 | | | | 0.4 | | 71.9 | 18,659 |
| F9 | 21,350 | 93.4 | | | | 0.1 | | 6.5 | 1,048 |
| F10 | 21,350 | 94.2 | | | | * | | 5.7 | 1,048 |
| F11A | 21,350 | 72.0 | | | | 1.7 | | 26.3 | 1,048 |
| F11B | 21,350 | 16.5 | | | | 1.1 | | 82.4 | 1,048 |
| (b) | (4,050) | (86.9) | | | | (4.6) | | (8.5) | (18,348) |
| F11C | 21,350 | 46.0 | | | | 1.1 | | 52.9 | 1,048 |
| (b) | (20,496) | (47.9) | | | | (1.2) | | (50.9) | (1,902) |
| F11D | 21,350 | 10.8 | | | | 0.8 | | 88.3 | 1,048 |
| (b) | (4,050) | (57.2) | | | | (3.1) | | (39.7) | (18,348) |
| F11E | 21,350 | 26.0 | | | | 0.7 | | 73.2 | 1,048 |
| (b) | (20,496) | (27.1) | | | | (0.8) | | (72.1) | (1,902) |
| F11F | 21,350 | 7.6 | | | | 0.6 | | 91.8 | 1,048 |
| (b) | (4,050) | (40.1) | | | | (3.3) | | (56.6) | (18,348) |
| F11G | 21,350 | 23.0 | | | | 0.8 | | 76.2 | 1,048 |
| (b) | (20,496) | (23.9) | | | | (0.8) | | (75.3) | (1,902) |
| F11H | 21,350 | 7.7 | | | | 0.7 | | 91.6 | 1,048 |
| (b) | (4,050) | (40.6) | | | | (3.5) | | (55.9) | (18,348) |
| F12 | 21,350 | 93.7 | | | | 0.3 | | 6.1 | 1,048 |

See notes at end of Table 3, pp. 145-147.

TABLE 3 (Cont'd)

RESPONSE DISTRIBUTIONS, SELECTED FIRST FOLLOW-UP AND BASE-YEAR ITEMS

| Item Number | Number Eligible to Answer (100%) | Usable Responses (%) | Unusable Responses: Routing-Error Codes 20 (%) | 40 (%) | 60 (%) | Unusable Responses: "Garbage" Codes[a] (94 to 97) (%) | Non-Response: PARTIAL RESPONSE (93) (%) | Non-Response: BLANK (98) (%) | LEGITSKIP (99) (number) |
|---|---|---|---|---|---|---|---|---|---|
| F13A | 21,350 | 89.6 | | | | 0.7 | | 9.7 | 1,048 |
| F13B | 21,350 | 83.2 | 0.1 | 0.9 | | * | | 13.2 | 1,048 |
| F13C | 7,519 | 62.4 | | | | 0.3 | | 37.3 | 14,879 |
| F14 | 21,350 | 92.5 | | | | 0.3 | | 7.1 | 1,048 |
| F16A | 21,350 | 92.8 | | | | 0.2 | 6.6 | 0.3 | 1,048 |
| F16B | 21,350 | 85.2 | | | | 0.3 | 14.3 | 0.3 | 1,048 |
| F16C | 21,350 | 88.9 | | | | 0.3 | 10.5 | 0.3 | 1,048 |
| F16D | 21,350 | 84.4 | | | | 0.2 | 15.1 | 0.3 | 1,048 |
| F16E | 21,350 | 84.8 | | | | 0.2 | 14.7 | 0.3 | 1,048 |
| F16F | 21,350 | 63.5 | | | | 0.4 | 35.9 | 0.2 | 1,048 |
| F19 | 21,350 | 91.0 | | | | 2.6 | | 6.4 | 1,048 |
| F21 | 21,350 | 98.0 | 1.3 | 0.3 | | * | | 0.5 | 1,048 |
| F22AA | 4,891 | 86.9 | | | | 0.2 | 8.5 | 4.4 | 17,507 |
| F22AB | 4,891 | 73.8 | | | | * | 21.7 | 4.5 | 17,507 |
| F22AC | 4,891 | 73.7 | | | | * | 21.8 | 4.5 | 17,507 |
| F22AD | 4,891 | 72.9 | | | | * | 22.6 | 4.5 | 17,507 |
| F22AE | 4,891 | 73.5 | | | | * | 22.0 | 4.5 | 17,507 |
| F22AF | 4,891 | 72.6 | | | | * | 22.9 | 4.5 | 17,507 |

See notes at end of Table 3, pp. 145-147.

TABLE 3 (Cont'd)

RESPONSE DISTRIBUTIONS, SELECTED FIRST FOLLOW-UP AND BASE-YEAR ITEMS

| Item Number | Number Eligible to Answer (100%) | Usable Responses (%) | Unusable Responses: Routing-Error Codes 20 (%) | 40 (%) | 60 (%) | Unusable Responses: "Garbage" Codes[a] (94 to 97) (%) | Non-Response: PARTIAL RESPONSE (93) (%) | Non-Response: BLANK (98) (%) | LEGITSKIP (99) (number) |
|---|---|---|---|---|---|---|---|---|---|
| F22AG | 4,891 | 73.4 | | | | * | 22.0 | 4.5 | 17,507 |
| F22AH | 4,891 | 74.7 | | | | * | 20.7 | 4.5 | 17,507 |
| F22AI | 4,891 | 68.1 | | | | * | 37.3 | 4.5 | 17,507 |
| F22B | 4,874 | 83.5 | | | | 0.6 | | 15.8 | 17,524 |
| F22C | 4,891 | 86.8 | | | | 0.4 | | 12.7 | 17,507 |
| F22D | 4,891 | 86.6 | | | | 0.6 | | 12.8 | 17,507 |
| F22E | 4,891 | 87.1 | | | | 0.1 | | 12.8 | 17,507 |
| F23 | 21,350 | 87.5 | 6.1 | 3.7 | 2.4 | * | | 0.3 | 1,048 |
| F24A | 8,118 | 79.8 | | | | 0.2 | 3.5 | 16.5 | 14,280 |
| F24B | 8,118 | 79.9 | | | | 0.2 | 3.4 | 16.5 | 14,280 |
| F24C | 8,118 | 79.5 | | | | 0.2 | 3.8 | 16.5 | 14,280 |
| F24D | 8,118 | 79.4 | | | | 0.2 | 3.9 | 16.5 | 14,280 |
| F24E | 8,118 | 79.4 | | | | 0.2 | 3.9 | 16.5 | 14,280 |
| F24F | 8,118 | 79.3 | | | | 0.2 | 4.0 | 16.5 | 14,280 |
| F24G | 8,118 | 79.3 | | | | 0.2 | 4.0 | 16.5 | 14,280 |
| F24H | 8,118 | 79.3 | | | | 0.2 | 4.1 | 16.5 | 14,280 |
| F24I | 8,118 | 79.2 | | | | 0.2 | 4.1 | 16.5 | 14,280 |
| F24J | 8,118 | 79.3 | | | | 0.2 | 4.1 | 16.5 | 14,280 |

See notes at end of Table 3, pp. 145-147.

TABLE 3 (Cont'd)

RESPONSE DISTRIBUTIONS, SELECTED FIRST FOLLOW-UP AND BASE-YEAR ITEMS

| Item Number | Number Eligible to Answer (100%) | Usable Responses (%) | Unusable Responses: Routing-Error Codes 20 (%) | 40 (%) | 60 (%) | Unusable Responses: "Garbage" Codes[a] (94 to 97) (%) | Non-Response: PARTIAL RESPONSE (93) (%) | Non-Response: BLANK (98) (%) | Non-Response: LEGITSKIP (99) (number) |
|---|---|---|---|---|---|---|---|---|---|
| F24K | 8,118 | 79.7 | | | | 0.2 | 3.6 | 16.5 | 14,280 |
| F24L | 8,118 | 79.6 | | | | 0.2 | 3.7 | 16.5 | 14,280 |
| F24M | 8,118 | 79.9 | | | | 0.2 | 3.4 | 16.5 | 14,280 |
| F24N | 8,118 | 79.0 | | | | 0.2 | 4.2 | 16.5 | 14,280 |
| F24O | 8,118 | 79.4 | | | | 0.2 | 3.9 | 16.5 | 14,280 |
| F24P | 8,118 | 80.0 | | | | 0.2 | 3.4 | 16.5 | 14,280 |
| F24Q | 8,118 | 78.8 | | | | 0.2 | 4.5 | 16.5 | 14,280 |
| F25 | 15,903 | 90.0 | 1.4 | 0.3 | | * | | 8.3 | 6,495 |
| F26B | 12,177 | 73.7 | | | | 0.3 | | 25.9 | 10,221 |
| F26C | 12,177 | 73.6 | | | | 0.1 | | 26.2 | 10,221 |
| F27AA | 12,177 | 73.3 | | | | 0.2 | | 26.6 | 10,221 |
| F27AB | 12,177 | 73.3 | | | | 0.2 | | 26.5 | 10,221 |
| F27B | 12,177 | 73.6 | | | | 0.1 | | 26.3 | 10,221 |
| F27C | 12,177 | 71.2 | | | | 0.8 | | 28.0 | 10,221 |
| F27D | 12,177 | 73.0 | | | | 0.3 | | 26.7 | 10,221 |
| F28A | 12,002 | 76.6 | | | | 0.1 | | 23.3 | 10,396 |
| F28B | 12,177 | 70.9 | 1.3 | 3.2 | | 0.3 | | 24.3 | 10,221 |
| F28C | 11,825 | 72.1 | | | | 0.4 | | 27.5 | 10,573 |



150 See notes at end of Table 3, pp. 145-147. 151

TABLE 3 (Cont'd)

RESPONSE DISTRIBUTIONS, SELECTED FIRST FOLLOW-UP AND BASE-YEAR ITEMS

| Item Number | Number Eligible to Answer (100%) | Usable Responses (%) | Unusable Responses: Routing-Error Codes 20 (%) | 40 (%) | 60 (%) | Unusable Responses: "Garbage" Codes[a] (94 to 97) (%) | Non-Response: PARTIAL RESPONSE (93) (%) | BLANK (98) (%) | LEGITSKIP (99) (number) |
|---|---|---|---|---|---|---|---|---|---|
| F29A | 15,903 | 85.5 | 2.8 | 1.9 | 1.4 | 0.1 | | 8.2 | 6,495 |
| F29BA | 5,051 | 55.6 | | | | 0.2 | 4.8 | 39.5 | 17,347 |
| F29BB | 5,051 | 54.5 | | | | 0.2 | 5.8 | 39.5 | 17,347 |
| F29BC | 5,051 | 53.8 | | | | 0.2 | 6.4 | 39.5 | 17,347 |
| F29BD | 5,051 | 53.6 | | | | 0.2 | 6.7 | 39.5 | 17,347 |
| F29BE | 5,051 | 53.3 | | | | 0.3 | 6.9 | 39.5 | 17,347 |
| F29BF | 5,051 | 53.3 | | | | 0.2 | 7.0 | 39.5 | 17,347 |
| F29BG | 5,051 | 53.3 | | | | 0.2 | 7.0 | 39.5 | 17,347 |
| F29BH | 5,051 | 53.3 | | | | 0.2 | 7.0 | 39.5 | 17,347 |
| F29BI | 5,051 | 53.3 | | | | 0.2 | 7.0 | 39.5 | 17,347 |
| F29BJ | 5,051 | 53.3 | | | | 0.2 | 7.0 | 39.5 | 17,347 |
| F29BK | 5,051 | 53.8 | | | | 0.2 | 6.4 | 39.5 | 17,347 |
| F29BL | 5,051 | 53.6 | | | | 0.3 | 6.6 | 39.5 | 17,347 |
| F29BM | 5,051 | 54.2 | | | | 0.3 | 6.0 | 39.5 | 17,347 |
| F29BN | 5,051 | 53.8 | | | | 0.2 | 6.5 | 39.5 | 17,347 |
| F29BO | 5,051 | 53.4 | | | | 0.3 | 6.8 | 39.5 | 17,347 |
| F29BP | 5,051 | 53.6 | | | | 0.2 | 6.7 | 39.5 | 17,347 |
| F29BQ | 5,051 | 54.4 | | | | 0.3 | 5.7 | 39.5 | 17,347 |
| F29BR | 5,051 | 53.2 | | | | 0.2 | 7.0 | 39.5 | 17,347 |

See notes at end of Table 3, pp. 145-147.

TABLE 3 (Cont'd)

RESPONSE DISTRIBUTIONS, SELECTED FIRST FOLLOW-UP AND BASE-YEAR ITEMS

| Item Number | Number Eligible to Answer (100%) | Usable Responses (%) | Unusable Responses: Routing-Error Codes 20 (%) | 40 (%) | 60 (%) | Unusable Responses: "Garbage" Codes[a] (94 to 97) (%) | Non-Response: PARTIAL RESPONSE (93) (%) | BLANK (98) (%) | LEGITSKIP (99) (number) |
|---|---|---|---|---|---|---|---|---|---|
| F30 | 14,077 | 77.8 | 3.1 | 2.5 | | * | | 16.5 | 8,321 |
| F31A | 4,884 | 27.7 | | | | 0.2 | 1.4 | 70.7 | 17,514 |
| F31B | 4,884 | 27.4 | | | | 0.2 | 1.7 | 70.7 | 17,514 |
| F31C | 4,884 | 27.5 | | | | 0.2 | 1.6 | 70.7 | 17,514 |
| F31D | 4,884 | 27.4 | | | | 0.2 | 1.7 | 70.7 | 17,514 |
| F31E | 4,884 | 27.3 | | | | 0.2 | 1.8 | 70.7 | 17,514 |
| F31F | 4,884 | 27.3 | | | | 0.2 | 1.7 | 70.7 | 17,514 |
| F31G | 4,884 | 27.2 | | | | 0.2 | 1.9 | 70.7 | 17,514 |
| F31H | 4,884 | 27.6 | | | | 0.2 | 1.4 | 70.7 | 17,514 |
| F31I | 4,884 | 27.4 | | | | 0.2 | 1.7 | 70.7 | 17,514 |
| F31J | 4,884 | 27.2 | | | | 0.2 | 1.8 | 70.7 | 17,514 |
| F31K | 4,884 | 27.3 | | | | 0.2 | 1.8 | 70.7 | 17,514 |
| F32B | 7,438 | 59.9 | | | | 0.4 | | 39.8 | 14,960 |
| F32C | 7,438 | 60.1 | | | | 0.4 | | 39.5 | 14,960 |
| F33B | 14,077 | 78.3 | | | | 0.2 | | 21.4 | 8,321 |
| F33C | 14,077 | 75.3 | | | | 1.2 | | 23.5 | 8,321 |
| F34 | 14,077 | 79.4 | | | | * | | 20.5 | 8,321 |
| F37 | 8,061 | 62.6 | | | | 0.2 | | 37.2 | 14,337 |

See notes at end of Table 3, pp. 145-147.

TABLE 3 (Cont'd)

RESPONSE DISTRIBUTIONS, SELECTED FIRST FOLLOW-UP AND BASE-YEAR ITEMS

| Item Number | Number Eligible to Answer (100%) | Usable Responses (%) | Unusable Responses: Routing-Error Codes 20 (%) | 40 (%) | 60 (%) | Unusable Responses: "Garbage" Codes[a] (94 to 97) (%) | Non-Response: PARTIAL RESPONSE (93) (%) | BLANK (98) (%) | LEGITSKIP (99) (number) |
|---|---|---|---|---|---|---|---|---|---|
| F39 | 15,903 | 85.6 | | | | * | | 14.3 | 6,495 |
| F40B | 5,221 | 31.8 | | | | 0.2 | | 68.0 | 17,177 |
| F40DA | 5,221 | 31.4 | | | | 0.1 | | 68.5 | 17,177 |
| F41B | 15,903 | 81.2 | | | | 0.2 | | 18.6 | 6,495 |
| F41CA | --- omitted --------see footnote "c"---------- | | | | | | | | |
| F41CB | 13,745 | 40.5 | | | | 1.9 | | 57.7 | 8,653 |
| F41CC | 13,739 | 13.8 | | | | 2.5 | | 83.7 | 8,659 |
| F42 | 15,903 | 86.6 | | | | * | | 13.3 | 6,495 |
| F43 | 15,903 | 87.3 | | | | * | | 12.6 | 6,495 |
| F46AA | 15,903 | 71.4 | | | | 1.9 | | 26.7 | 6,495 |
| F46AB | 15,903 | 68.4 | | | | 2.5 | | 29.1 | 6,495 |
| F46BA | 15,903 | 59.5 | | | | 1.9 | | 38.6 | 6,495 |
| F46BB | 15,903 | 40.4 | | | | 1.9 | | 57.8 | 6,495 |
| F46BC | 15,903 | 58.6 | | | | 2.0 | | 39.4 | 6,495 |
| F46BD | 15,903 | 46.1 | | | | 2.6 | | 51.3 | 6,495 |
| F46BE | 15,903 | 40.9 | | | | 2.4 | | 56.6 | 6,495 |
| F47AA | 15,889 | 66.3 | | | | 0.6 | | 33.1 | 6,509 |

See notes at end of Table 3, pp. 145-147.

TABLE 3 (Cont'd)

RESPONSE DISTRIBUTIONS, SELECTED FIRST FOLLOW-UP AND BASE-YEAR ITEMS

| Item Number | Number Eligible to Answer (100%) | Usable Responses (%) | Unusable Responses: Routing-Error Codes 20 (%) | 40 (%) | 60 (%) | Unusable Responses: "Garbage" Codes[a] (94 to 97) (%) | Non-Response: PARTIAL RESPONSE (93) (%) | BLANK (98) (%) | LEGITSKIP (99) (number) |
|---|---|---|---|---|---|---|---|---|---|
| F47AB | 15,889 | 64.8 | | | | 2.2 | | 33.1 | 6,509 |
| F47BA | 12,068 | 55.8 | | | | 0.6 | | 43.5 | 10,330 |
| F47BB | 12,068 | 54.2 | | | | 2.2 | | 43.5 | 10,330 |
| F47CA | 8,407 | 36.7 | | | | 0.9 | | 62.5 | 13,991 |
| F47CB | 8,407 | 35.8 | | | | 1.7 | | 62.5 | 13,991 |
| F47DA | 6,646 | 19.9 | | | | 1.0 | | 79.0 | 15,752 |
| F47DB | 6,646 | 19.6 | | | | 1.3 | | 79.0 | 15,752 |
| F47EA | 5,860 | 9.1 | | | | 1.1 | | 89.6 | 16,538 |
| F47EB | 5,860 | 9.0 | | | | 1.4 | | 89.6 | 16,538 |
| F47FA | 5,484 | 2.9 | | | | 1.3 | | 95.8 | 16,914 |
| F47FB | 5,484 | 2.8 | | | | 1.4 | | 95.8 | 16,914 |
| F47GA | 5,355 | 0.6 | | | | 1.3 | | 98.1 | 17,043 |
| F47GB | 5,355 | 0.6 | | | | 1.3 | | 98.1 | 17,043 |
| F48A | 21,350 | 95.0 | 1.3 | 2.8 | 0.4 | * | | 0.4 | 1,048 |
| F48C | 8,072 | 88.6 | | | | * | | 11.3 | 14,326 |
| F49A | 14,306 | 94.0 | | | | 0.5 | | 5.6 | 8,092 |
| F49G | 14,306 | 91.6 | | | | 0.1 | | 8.2 | 8,092 |



See notes at end of Table 3, pp. 145-147.

TABLE 3 (Cont'd)

RESPONSE DISTRIBUTIONS, SELECTED FIRST FOLLOW-UP AND BASE-YEAR ITEMS

| Item Number | Number Eligible to Answer (100%) | Usable Responses (%) | Unusable Responses: Routing-Error Codes 20 (%) | 40 (%) | 60 (%) | Unusable Responses: "Garbage" Codes[a] (94 to 97) (%) | Non-Response: PARTIAL RESPONSE (93) (%) | BLANK (98) (%) | LEGITSKIP (99) (number) |
|---|---|---|---|---|---|---|---|---|---|
| F50A | 14,306 | 82.8 | | | | 5.5[d] | | 11.7 | 8,092 |
| F50B | 14,306 | 84.6 | | | | 1.7 | | 13.6 | 8,092 |
| F54A | 21,350 | 91.4 | 1.7 | 5.4 | 0.6 | * | | 0.8 | 1,048 |
| F54C | 9,968 | 86.6 | | | | 0.1 | | 13.3 | 12,430 |
| F55A | 7,983 | 86.5 | | | | 0.6 | | 13.0 | 14,415 |
| F56A | 12,780 | 79.5 | | | | 5.3[e] | | 15.2 | 9,618 |
| F56B | 12,780 | 81.6 | | | | 1.6 | | 16.7 | 9,618 |
| F58A | 21,350 | 96.8 | | | | 1.0 | | 2.1 | 1,048 |
| F58B | 21,350 | 88.7 | | | | 0.7 | | 10.7 | 1,048 |
| F58C | 21,350 | 93.0 | | | | 0.4 | | 6.6 | 1,048 |
| F78A | 21,350 | 92.3 | | | | 0.6 | | 7.1 | 1,048 |
| F78B | 21,350 | 92.9 | | | | 0.4 | | 6.7 | 1,048 |
| F79 | 21,350 | 87.0 | | | | 2.4 | | 9.7 | 1,048 |
| F80A | 21,350 | 90.1 | | | | 2.7[f] | | 7.2 | 1,048 |
| F80B | 21,350 | 89.3 | | | | 2.8[f] | | 7.8 | 1,048 |
| F80C | 21,350 | 88.0 | | | | 3.7[f] | | 8.4 | 1,048 |
| F81 | 21,350 | 90.5 | 0.8 | 0.4 | | 0.1 | | 8.1 | 1,048 |
| F82B | 11,769 | 83.5 | | | | 0.4 | | 16.1 | 10,629 |

See notes at end of Table 3, pp. 145-147.

TABLE 3 (Cont'd)

RESPONSE DISTRIBUTIONS, SELECTED FIRST FOLLOW-UP AND BASE-YEAR ITEMS

| Item Number | Number Eligible to Answer (100%) | Usable Responses (%) | Unusable Responses: Routing-Error Codes 20 (%) | 40 (%) | 60 (%) | Unusable Responses: "Garbage" Codes[a] (94 to 97) (%) | Non-Response: PARTIAL RESPONSE (93) (%) | BLANK (98) (%) | LEGITSKIP (99) (number) |
|---|---|---|---|---|---|---|---|---|---|
| F82C | 11,769 | 80.8 | 0.4 | 1.3 | | 0.1 | | 17.3 | 10,629 |
| F82DA | 4,410 | 32.6 | | | | 1.8 | | 65.6 | 17,988 |
| F82DB | 4,410 | 31.7 | | | | 1.6 | | 66.7 | 17,988 |
| F82DC | 4,410 | 18.3 | | | | 1.8 | | 79.9 | 17,988 |
| F83AA | 11,769 | 75.2 | | | | 0.2 | | 24.6 | 10,629 |
| F83B | 6,428 | 64.0 | | | | 0.8 | | 35.2 | 15,970 |
| F83C | 6,428 | 64.4 | | | | 0.3 | | 35.3 | 15,970 |
| F83DA | 3,203 | 16.4 | | | | 1.7 | | 81.9 | 19,195 |
| F83DB | 3,203 | 12.0 | | | | 1.3 | | 86.6 | 19,195 |
| F83DC | 3,203 | 7.6 | | | | 1.4 | | 91.0 | 19,195 |
| BY2 | 21,222 | 97.0 | | | | | | 3.0 | 1,176 |
| BY5 | 21,222 | 97.8 | | | | | | 2.2 | 1,176 |
| BY8 | 21,222 | 97.8 | | | | | | 2.2 | 1,176 |
| BY93 | 21,222 | 78.1 | | | | | | 21.9 | 1,176 |
| BY94A | 21,222 | 95.9 | | | | | | 4.1 | 1,176 |
| BY94B | 21,222 | 96.5 | | | | | | 3.5 | 1,176 |
| BY94C | 21,222 | 96.6 | | | | | | 3.3 | 1,176 |
| BY94D | 21,222 | 96.5 | | | | | | 3.5 | 1,176 |

See notes at end of Table 3, pp. 145-147.

TABLE 3 (Cont'd)

RESPONSE DISTRIBUTIONS, SELECTED FIRST FOLLOW-UP AND BASE-YEAR ITEMS

| Item Number | Number Eligible to Answer (100%) | Usable Responses (%) | Unusable Responses: Routing-Error Codes 20 (%) | 40 (%) | 60 (%) | Unusable Responses: "Garbage" Codes[a] (94 to 97) (%) | Non-Response: PARTIAL RESPONSE (93) (%) | Non-Response: BLANK (98) (%) | Non-Response: LEGITSKIP (99) (number) |
|---|---|---|---|---|---|---|---|---|---|
| BY94E | 21,222 | 96.4 | | | | | | 3.6 | 1,176 |
| BY94F | 21,222 | 96.5 | | | | | | 3.5 | 1,176 |
| BY94G | 21,222 | 95.8 | | | | | | 4.2 | 1,176 |
| BY94H | 21,222 | 95.9 | | | | | | 4.1 | 1,176 |
| BY94I | 21,222 | 96.0 | | | | | | 4.0 | 1,176 |
| BY94J | 21,222 | 95.0 | | | | | | 5.0 | 1,176 |
| BY94K | 21,222 | 96.2 | | | | | | 3.8 | 1,176 |
| BY84 | 21,222 | 98.2 | | | | | | 1.8 | 1,176 |
| BY92 | 21,222 | 96.2 | | | | | | 3.8 | 1,176 |
| BY95 | 21,222 | 96.3 | | | | | | 3.7 | 1,176 |
| BY83 | 21,222 | 96.7 | | | | | | 3.3 | 1,176 |
| BY90A[g] | 16,683 | 84.2 | | | | 3.2 | | 12.6 | 5,715 |
| BY90B[g] | 16,683 | 86.2 | | | | 2.1 | | 11.8 | 5,715 |

NOTES: Cells without entries indicate no cases in category; cells marked by asterisk (*) had cases totalling less the 0.1 per cent. Percentages may not add to 100 due to rounding.

TABLE 3 (Cont'd)

RESPONSE DISTRIBUTIONS, SELECTED FIRST FOLLOW-UP AND BASE-YEAR ITEMS

NOTES: (Cont'd) "Usable responses" includes all cases tabulated in within-limits, specific coding categories. "Unusable responses" includes all cases which are not interpretable, beyond acceptable value limits, or whose validity is questioned owing to routing-pattern errors. "LEGITSKIP" includes cases not expected (not eligible) to answer the item. See the discussion of routing-error codes for some qualifications regarding the "usable" and "unusable" designations.

Items designated "F--" are from the First Follow-up Questionnaire only. Items designated "BY--" are basic background data for which information was collected from about 80 per cent of the respondents via the Base Year Questionnaire. This information was obtained from 4,539 respondents via First Follow-up Questionnaire, Form B, items 86-99. RTI has merged the latter data with the Base Year data in reporting distributions. High response rates for "BY--" items are probably attributable to the supervised data collection procedure used with the Base Year Questionnaire.

a "Garbage" codes are "Don't Know" (94), "Out of Range" (95), "Multiple Response" (96), "Refused Answer" (97), plus cases judged outside reasonable limits for free-response numerical items by RTI.

b Figures in parentheses represent estimates for the preceding item, based on the revised number of eligibles shown. See text p. 31 for discussion of the downward revision for "Spouse 1973 income" (items F11B, D, F, H) and "Respondent's 1973 income" (F11C, E, G).

c F41CA distribution omitted due to apparent tabulation error in published data. OUT OF RANGE (code 95) is listed with 19,947 cases.

d Responses judged outside reasonable limits by RTI account for most (4.8 per cent) of these "garbage coded" responses.

TABLE 3 (Cont'd)

[e] Responses judged outside reasonable limits by RTI account for most (4.2 per cent) of these "garbage coded" responses.

[f] Includes "does not apply," with n=515 (2.4 per cent) for F80A, n=542 (2.5 per cent) for F80B, and n=704 (3.3 per cent) for F80C.

[g] Items BY90A and BY90B are (respectively) Father's and Mother's education, as collected in the Base Year Questionnaire only. Included here for comparison with items F78A and F78B, which represent the same variables as collected via the First Follow-up Questionnaire. Categories for the two do not match exactly. "Garbage code" cases for BY90A and B are "does not apply" responses.

Source: National Longitudinal Study of the High School Cass of 1972: Base Year and First Follow-up Data File User's Manual (Preliminary). Research Triangle Park, N.C.: Research Triangle Institute, April 1975.

TABLE 4

IMPACT OF ALLOCATION ON 1970 SCHOOL ENROLLMENT DISTRIBUTION

| Levels (School level in which currently enrolled) | Distribution | | | | Change | Proportionate Change |
|---|---|---|---|---|---|---|
| | Without Allocation | | With Allocation | | | |
| | (1) Number (1,000s) | (2) Per Cent a/ | (3) Number (1,000s) | (4) Per Cent a/ | (5) (Col. 2 - Col. 4) | (6) (Col. 5/Col. 2) |
| Nursery School | 906.4 | 1.6 | 952.8 | 1.6 | 0 | 0% |
| Kindergarten | 2,945.7 | 5.2 | 3,022.4 | 5.1 | -0.1 | -1.9% |
| Elementary School | 31,794.8 | 56.3 | 33,210.2 | 56.5 | 0.2 | 0.3% |
| High School | 13,974.6 | 24.7 | 14,480.6 | 24.6 | -0.1 | -0.4% |
| College | 6,865.8 | 12.2 | 6,966.0 | 11.8 | -0.4 | -3.0% |
| Total Reported | 56,487.1 | 100.0 | 58,632.1 | 100.0 | | |
| Total Not Reported | 2,147.9 | | | | | |
| Per Cent Allocated | | | 3.7 Per Cent | | | |
| Average | | | | | 0.16[b] | 1.1%[b] |

[a] Base is Total Reported. May not add to 100 due to rounding.

[b] Simple average of entries, disregarding sign.

Source: Bureau of the Census. Census of Population: 1970; Vol. 1, Characteristics of the Population. Part 1, United States Summary--Sec. 2. Washington: GPO, 1973. Appendix C, pp. 68-69; ref. Table C-3 (p. 1-572), Table 197 (p. 1-605).

TABLE 5

IMPACT OF ALLOCATION ON 1969
"WEEKS WORKED" DISTRIBUTION

| Levels (Number of weeks worked in 1969, employed persons aged 16 and over) | Distribution | | | | Change | Proportionate Change |
|---|---|---|---|---|---|---|
| | Without Allocation | | With Allocation | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | Number (1,000s) | Per Cent a/ | Number (1,000s) | Per Cent a/ | (Col. 2 - Col. 4) | (Col. 5/Col. 2) |
| 50 - 52 | 50,188.1 | 58.6 | 53,662.0 | 58.1 | -0.5 | -0.8% |
| 48 - 49 | 4,978.1 | 5.8 | 5,397.0 | 5.8 | 0 | 0% |
| 40 - 47 | 7,256.4 | 8.5 | 7,877.7 | 8.5 | 0 | 0% |
| 27 - 39 | 7,198.2 | 8.4 | 7,851.1 | 8.5 | 0.1 | 1.2% |
| 14 - 26 | 7,028.4 | 8.2 | 7,709.4 | 8.3 | 0.1 | 1.2% |
| 13 or less | 8,981.0 | 10.5 | 9,912.8 | 10.7 | 0.2 | 1.9% |
| Total Reported | 85,630.3 | 100.0 | 92,410.0 | 100.0 | | |
| Total Not Reported | 9,145.8 | | | | | |
| Per Cent Allocated | | | 7.3 Per Cent | | | |
| Average | | | | | 0.15[b] | 0.8%[b] |

a Base is Total Reported. May not add to 100 due to rounding.

b Simple average of entries, disregarding sign.

Source: Same as Table 1, except Census tables C-3, p. 1-573 and 218, p. 1-702.

TABLE 6

IMPACT OF ALLOCATION ON 1970 EDUCATIONAL ATTAINMENT DISTRIBUTION

| Levels (Highest grade completed, persons aged 25 or older) | Distribution: Without Allocation (1) Number (1,000s) | Distribution: Without Allocation (2) Per Cent a/ | Distribution: With Allocation (3) Number (1,000s) | Distribution: With Allocation (4) Per Cent a/ | Change (5) (Col. 2 - Col. 4) | Proportionate Change (6) (Col. 5/Col. 2) |
|---|---|---|---|---|---|---|
| None | 1,733.9 | 1.7 | 1,767.7 | 1.6 | -0.1 | -5.8% |
| Elementary: | | | | | | |
| 1 - 4 | 3,794.7 | 3.7 | 4,271.6 | 3.9 | 0.2 | 5.4% |
| 5 - 6 | 5,542.4 | 5.5 | 6,217.1 | 5.7 | 0.2 | 3.6% |
| 7 | 4,339.0 | 4.3 | 4,815.6 | 4.4 | 0.1 | 2.3% |
| 8 | 12,816.5 | 12.6 | 14,015.4 | 12.8 | 0.2 | 1.6% |
| High School: | | | | | | |
| 1 - 3 | 19,407.0 | 19.1 | 21,285.9 | 19.4 | 0.3 | 1.6% |
| 4 | 32,138.9 | 31.7 | 34,158.1 | 31.1 | -0.6 | -1.9% |
| College: | | | | | | |
| 1 - 3 | 10,748.2 | 10.6 | 11,650.7 | 10.6 | 0 | 0% |
| 4 | 6,265.4 | 6.2 | 6,657.6 | 6.1 | -0.1 | -1.6% |
| 5 or more | 4,689.4 | 4.6 | 5,059.7 | 4.6 | 0 | 0% |

TABLE 6 (Cont'd)

IMPACT OF ALLOCATION ON 1970 EDUCATIONAL ATTAINMENT DISTRIBUTION

| Levels | Distribution | | | | Change | Proportionate Change |
|---|---|---|---|---|---|---|
| | Without Allocation | | With Allocation | | | |
| (Highes grade completed, persons aged 25 or older) | (1) Number (1,000s) | (2) Per Cent a/ | (3) Number (1,000s) | (4) Per Cent a/ | (5) (Col. 2 - Col. 4) | (6) (Col. 5/Col. 2) |
| Total Reported | 101,475.3 | 100.0 | 109,899.4 | 100.0 | | |
| Total Not Reported | 8,424.1 | | | | | |
| Per Cent Allocated | | | 7.7 Per Cent | | | |
| Average | | | | | 0.18[b] | 2.4%[b] |

a Base is Total Reported. May not add to 100 due to rounding.

b Simple average of entries, disregarding sign.

Source: Same as Table 1, except Census Tables C-3, p. 1-572 and 199, p. 1-627.

TABLE 7

IMPACT OF ALLOCATION ON 1969 FAMILY INCOME DISTRIBUTION

| Levels (1969 Family income, in dollars) | Distribution: Without Allocation (1) Number of Families (1,000s) | Distribution: Without Allocation (2) Per Cent a/ | Distribution: With Allocation (3) Number of Families (1,000s) | Distribution: With Allocation (4) Per Cent a/ | Change (5) (Col. 2 - Col. 4) | Proportionate Change (6) (Col. 5/ Col. 2) |
|---|---|---|---|---|---|---|
| less than $1,000 | 918.7 | 2.3 | 1,276.7 | 2.5 | 0.2 | 8.7% |
| 1,000 - 1,999 | 1,324.4 | 3.3 | 1,734.3 | 3.4 | 0.1 | 3.0% |
| 2,000 - 2,999 | 1,749.8 | 4.3 | 2,261.9 | 4.4 | 0.1 | 2.3% |
| 3,000 - 3,999 | 1,938.3 | 4.8 | 2,501.2 | 4.9 | 0.1 | 2.1% |
| 4,000 - 4,999 | 2,021.9 | 5.0 | 2,603.3 | 5.1 | 0.1 | 2.0% |
| 5,000 - 5,999 | 2,307.4 | 5.7 | 2,936.1 | 5.7 | 0 | 0% |
| 6,000 - 6,999 | 2,497.7 | 6.2 | 3,148.1 | 6.2 | 0 | 0% |
| 7,000 - 7,999 | 2,776.9 | 6.8 | 3,453.4 | 6.7 | -0.1 | -1.5% |
| 8,000 - 8,999 | 2,952.4 | 7.3 | | | | |
| 9,000 - 9,999 | 2,815.0 | 6.9 | 7,102.8 | 13.9 | -0.3 | -2.1% |
| 10,000 - 11,999 | 5,377.9 | 13.2 | | | | |
| 12,000 - 14,999 | 5,709.3 | 14.1 | 13,625.7 | 26.6 | -0.7 | -2.6% |
| 15,000 - 24,999 | 6,442.0 | 15.9 | 8,182.6 | 16.0 | 0.1 | 0.6% |
| 25,000 - 49,999 | 1,467.4 | 3.6 | 1,974.8 | 3.9 | 0.3 | 8.3% |
| 50,000 or more | 290.3 | 0.7 | 367.6 | 0.7 | 0 | 0% |

TABLE 7 (Cont'd)

IMPACT OF ALLOCATION ON 1969 FAMILY INCOME DISTRIBUTION

| Levels (1969 Family income, in dollars) | Distribution | | | | Change | Proportionate Change |
|---|---|---|---|---|---|---|
| | Without Allocation | | With Allocation | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | Number of Families (1,000s) | Per Cent a/ | Number of Families (1,000s) | Per Cent a/ | (Col. 2- Col. 4) | (Col. 5/Col. 2) |
| Total Reported | 40,589.5 | 100.0 | 51,168.6 | 100.0 | | |
| Total Not Reported | 10,579.1 | | | | | |
| Per Cent Allocated | | | 20.7 Per Cent | | | |
| Average | | | | | 0.16[b] | 2.6%[b] |

[a] Base is Total Reported. May not add to 100 due to rounding.

[b] Simple average of entries, disregarding sign.

Source: Same as Table 1, except Census Tables C-3, p. 1-574 and 252, p. 1-923.